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MEMOIRS AND PROCEEDINGS

OF THE

MANCHESTER LITERARY & PHILOSOPHICAL SOCIETY.

(MANCHESTER MEMOIRS.)

VOLUME LV. (1910-11.)

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Manchester Memoirs, Vol. lv. (1910), *No.* 1.

I. On the Origin of Cometary Bodies and Saturn's Rings.

By Henry Wilde, D.Sc., D.C.L., F.R.S.

Received and read October 4th, 1910.

As the first Halley Lecture which I delivered before the University of Oxford in May last* contained some matters new to astronomical science, it has appeared to me that an abridgment of the lecture, with some additions which have since presented themselves to me, would be of value in continuation of my papers recently published by the Society.†

While the principle of dualism is abundantly manifest in every department of knowledge and fully recognized in the attractions and repulsions in molecular physics, the phenomena of the repulsive energy of celestial bodies have so far been unduly obscured by the more general principles of moving force and the attraction of gravitation.

The doctrine that the solar system, as at present constituted, was formed by the successive condensations of a nebular substance rotating about a central position, has been more firmly established during recent years through the great advances made in stellar photography, by which many of the nebulæ are visualized in various stages of evolution as right- and left-handed spirals, and clearly indicating the direction of their revolutions.‡

* Clarendon Press. Frowde. 1910.

† *Manchester Memoirs*, vols. 53, 54, 1909, 1910.
Phil. Mag. (6), vols. 18, 19, 1909, 1910.

‡ "Celestial Photographs," by Isaac Roberts, F.R.S. Vols. 1, 2, 1893. 1899.

November 11th, 1910.

The more interesting of these nebulæ are, M.31 Andromedæ, M.51 Canum, M.100 Comæ, M.74 Piscium, and many others from which the origin of planetary systems may be inferred with the same degree of probability as in the historical sequences observable in chemistry, geology, biology, or in any other department of the natural sciences.

That the subsequent condensations of planetary nebulæ into spherical bodies would be attended by the evolution of an amount of heat sufficient to make them vividly incandescent, is an obvious conclusion drawn directly from experimental science. It will be further evident that, after the heat of compression had attained its maximum, the self-luminous planets would ultimately become dark bodies through the radiation of their heat into free space.

It is very generally admitted that the sun, notwithstanding his vast dimensions, would, by continuous loss of heat, ultimately become a dark body like each member of the planetary system. It is also known that the internal parts of the sun are in a gaseous condition and under immense pressure. Some idea of the repulsive force exercised by this pressure may be formed from the ejection of enormous masses of incandescent gas from the surface of the sun to the height of 200,000 miles, with an estimated velocity of 166 miles per second.*

Assuming the secular cooling of the sun to be continuous, the liquefaction and final solidification of his outward parts would follow in natural sequence in accordance with common experience of cooling bodies, while the central parts would remain in their primitive gaseous condition. From strict analogy, it may justly be inferred that all the planetary bodies have gone through the same stages of

* Young, *American Journal of Science*, 1871, p. 468.

cooling as those outlined in the instance of the central body.

The notion that the earth and, inferentially, the other planets are solid bodies throughout, finds no support from a reasonable consideration of the constituents of the earth's crust, so far as they are accessible to observation. The late distinguished Professor of Geology in Oxford University (Sir Joseph Prestwich), in his classical work on Chemical, Physical, and Stratigraphical Geology, has clearly demonstrated from the uplift of continental areas and mountain chains, the welling out of basaltic lavas over many thousand square miles of surface and of great thickness, that a comparatively thin crust enveloping a fluid interior is a necessary condition to satisfy the requirements of geologists and physicists. More significant still is the succession of foldings of the earth's crust and stratigraphic contortions of small curvature, both of which features indicate a thickness of solid crust less than twenty-five miles. How far the imprisoned gases at the centre of the earth and the aqueous vapours near the surface may have contributed respectively to produce these geological changes, it is unnecessary now to discuss, but in the instance of the moon, which has neither water nor an atmosphere, the evidence of intense volcanic action manifested on its surface can only be accounted for by the ejective force of the gaseous substances in its interior, similar to that by which the incandescent gases from the surface of the sun are projected.

The fine series of photographic enlargements of the moon executed by MM. Loewy and Puiseux, of the Paris Observatory, show the greater part of its surface, from the equator to the poles, covered with extinct volcanoes in every stage of formation, similar to those on the terrestrial globe. Some of these volcanoes are twelve

thousand feet in height, with their craters upwards of forty miles in diameter, and are striking evidence of the immense repulsive force which produced them.

It has for a long time been considered on good evidence that the planetoids between the orbits of Mars and Jupiter (now numbering more than 600) are the fragments of a large planet which had formerly revolved in an orbit about the same distance from the sun as Ceres, and had been shattered by some internal convulsion. This hypothesis was put forward by Olbers the discoverer of Pallas in 1802, and was made the subject of a memoir by Lagrange in which he determined the explosive force necessary to detach a fragment of a planet that would cause it to describe the orbit of a comet. The nebulosities of the dense atmospheres of some of these planetoids concealing their disks indicate an incipient change of planetary into cometary bodies.

Attempts have been made during recent years to discredit the explanation offered by Olbers of the origin of the planetoids, by assuming that the annulus or convolute of nebular substance failed to resolve itself into a sphere, but was broken up into a number of small bodies.

There is no inherent improbability in the idea of a nebular convolution resolving itself into a number of discrete spherical bodies as many of such are to be seen in the convolutions of spiral nebulæ, of which M.100 Comæ and M.74 Piscium are the most striking examples. The convolutions of these nebulæ contain nebular stars which are involved symmetrically and follow the curvature of the convolutions. M.100 Comæ is further interesting from the fact of its showing elongated fissions of the convolutions previous to their development into spherical bodies. Such discrete bodies, revolving in a circular orbit of the same diameter would, by their mutual

attractions, ultimately coalesce to form a single planet, as postulated in my paper in connexion with the contraction of the radius vector of Neptune.*

As the orbits of all the planets are nearly in the plane of the ecliptic, and also of comparatively small eccentricity, it would become necessary to further assume that all the rings of discrete bodies should revolve in the same plane of the ecliptic, and in orbits nearly circular as do the other planetary bodies; but Olbers found that Pallas had the large orbital inclination of 34°·7, and many others are inclined from 26 to 15 degrees.

The eccentricities of some of the planetoids are also very large, that of Æthra being 0·380, Juno 0·257, and Pallas 0·238. The periodic times vary between 7·86 years (Hilda) and 1·75 years (Eros) with the correlated large differences in their mean distances from the sun; Hilda being 3·95 astronomical units, and Eros only 1·46 units which thereby intersects the orbit of Mars, 1·52 units.

The large differences observable in the elements of the planetoids, clearly indicate them as fragments of a large planet, in accordance with the conclusions arrived at by Olbers in 1802. The illustrious astronomer further assumed that the orbits of all the fragments would intersect each other at the point where the explosion occurred. Subsequent observations have, however, shown (which I shall confirm further on) that this supposition, while applicable in many instances, does not hold good as a generalization.

It will now be evident, without further discussion, that had the exploded major planet been a solid body throughout as hard as steel, it would still be revolving in

* *Manchester Memoirs*, vol. 54, 1910.
Phil. Mag., (6) vol. 19, 1910, p. 604.

its orbit, and would thus have deprived the world of an interesting chapter of astronomical science.

A review of the history of cometary astronomy brings out the remarkable fact that, while much has been written on the nature and motions of comets, few, if any, serious attempts have been made to account for their origin. The general opinion of modern astronomers, in accordance with the views of Kant* and Laplace,† is that these bodies are strangers to the solar system, which have been captured in the course of their lawless wanderings from the depths of the stellar universe.

The principal objection to this supposition is the immense distance of the solar system from the fixed stars. The best determination of the distance of the nearest of them was made by Dr. Gill at the Cape of Good Hope in 1881, which showed that α Centauri had a parallax of 0·75″, indicating a distance of about 25 billion miles, or 9,000 million miles more distant from Neptune than that planet is from the sun. As the attraction of gravitation at the orbit of Neptune is only one forty-second millionth of that at the solar surface, the attractive force at the distance of the fixed stars may be considered a negligible quantity in determining the motions of cometary bodies having their origin in other planetary systems. Granting for the moment that comets actually belong to other stellar systems, the problem of their origin and formation would still present itself for solution to earnest inquirers into the nature and causes of things.

The discoveries in cometary astronomy, more especially those of Schiaparelli, that the orbits of certain comets are identical with those of well-known streams of

* Kant's "Natural History and Theory of the Heavens," Chapter 3.

† Laplace's "Système du Monde," 1824.

meteors, as instanced in the comets of Tempel and of Biela in relation to the November meteors, clearly point to the conclusion that the place of origin of these erratic bodies is within the confines of the solar system, and that they have, consequently, always been members of it. Moreover, all meteoric bodies, as is well known, are mechanical mixtures of elementary substances or their compounds, and further indicate them as the ejectamenta of planetary bodies.

That comets are planetary ejectamenta, principally from the larger planets, may be justly inferred from the prodigious force manifested by the ejections from other celestial bodies to which attention has already been directed.

The determining cause of the ejection of a comet from any planet would be found in the conjunctive attractions of one or more of their number acting upon that part of the surface from which the cometary matter was ejected. The orbital direction of a comet would be determined solely by the position of the breach in the crust in relation to the orbital motion at the moment of discharge. The motion would be *direct* when its discharge coincided with the orbital motion of the planet, and *retrograde* when it was in the opposite direction, as shown in the annexed plate. And, according as the discharge was more or less at right angles to the plane of the planetary orbit, so would the angular direction of the comet in relation to the ecliptic be determined. The discharge of cometary bodies from vents in high planetary latitudes would necessarily have the greatest inclination to the ecliptic. It may be observed in this connexion that some of the large craters on the moon's surface, and of the terrestrial active volcanoes, Hecla and Mount Erebus, are also in high latitudes.

To those who are not familiar with the problems of experimental mechanics, it may be of some advantage to demonstrate more fully the direct and retrograde motions of cometary bodies by further illustrations than those shown in my Halley lecture.

It is common knowledge, based on well-established observations, that the axial and orbital rotations of all the planets are in the same direction, the sun also revolving on its axis in the same direction as the planets.

As a consequence of the common direction of the axial rotations, the adjoining circumferential parts revolve in opposite directions to each other, as will be seen in the annexed diagram of the Sun and Jupiter. Hence, while the circumferential parts of the planets next to the sun revolve from west to east, the sun apparently revolves from east to west, as is manifest from the motion of the dark spots across the solar disk.

That the circumferences of moving circles rotate about their centres in contrary directions at opposite extremities of their diameters is an axiomatic truth which finds its concrete expression in the diagram referred to. This geometrical relation is also practically illustrated in the reaction steam engine of Hero of Alexandria, in which a hollow globe is made to revolve by two jets of steam issuing in contrary directions from opposite extremities of its diameter. Other instances of direct and retrograde motion may also be seen in the catharine wheels of ordinary firework displays, and in hydraulic turbines with multiple jets around their circumferences.

Halley's original conception of concentric spheres rotating within the earth, with a differential motion, is fruitful in leading to the further idea that the ejection of comets from a planet may be periodic from causes within

itself, in like manner to the eleven years maximum sun-spot ejections of elementary gaseous substances. For it is only necessary to assume that, after the ejection of cometary matter through the double thickness of two concentric shells, the differential motion would retard, or wholly prevent, the further discharge until the vents were again coincident.

The planet Jupiter, from his vast dimensions, is the most interesting member of the solar system for the study of planetary and cometary evolution. The great red spot on his surface is generally considered to be caused by luminous vapours at great depths within the globe, if not by the actual incandescent crust of that part of the planet. The great extent and permanency of this spot indicate it as the locus of one of the vents through which comets and cometary satellites have been ejected at different periods of the history of the planet.

It is now generally recognized that certain groups of periodic comets are associated in some way unknown with the larger planets respectively ; the comets of short period belonging to Jupiter, as nearest to the sun, and the long period comets (of which Halley's is the most notable member) to Neptune and intermediate planets.

All the motions of periodic comets are well explained on the assumption of their moving in elliptical orbits more or less elongated, but the vast tabulated periodic times of comets supposed to move in parabolic and hyperbolic curves are necessarily ultra-speculative.

As the attraction of solar gravitation extends far beyond the orbit of Neptune, the motion of a body on the line of an open curve would ultimately be arrested and a comet would necessarily return over the same track, approximately, with a retrograde motion as an unknown

member of the solar system. Halley's comet, however, is considered to move in an elliptical orbit and has, therefore, the longest periodic time of which astronomers have certain knowledge.

As the principle of conservation holds good alike for celestial and terrestrial bodies, the moving force of comets will not exceed the attraction of gravitation beyond the limits of the solar system, and will be much less through the conversion of molar into molecular motion by friction of the discrete particles of cometary matter among themselves during the act of ejection, as also from the resistance of the medium through which they move in their orbits, and especially near the sun.

The principle of conservation, as will be obvious, will hold equally for the comets ejected from the planets of other stellar systems. Hence the absurdity of bringing cometary bodies into the solar system which contains within itself the power of evolving its own comets. Moreover, it will be further evident that this immigration notion might be extended to include the Earth and other planets as bodies from other stellar systems, captured by the Sun in their wanderings from outer space.

Jupiter, with his system of satellites, is generally regarded as a miniature solar system formed by the successive condensations of a nebular substance surrounding the planet. The laws of attraction, moving force, and Kepler's laws have the same relations among his satellites as in the planetary system. The binary progression of the periodic times of the three adjoining major satellites, Io, Europa, and Ganymede (which are very nearly in the ratio of 1, 2, 4) indicates an orderly process of evolution similar to that of the binary progression of the planetary distances.

The erratic movements and irregular orbits of the three outer Jovian satellites recently discovered have, however, presented a new problem for solution in connexion with the nebular theory of the evolution of satellites, as it was found that the orbital motion of the outermost one was in a retrograde direction.

An attempt has been made to explain the anomaly by assuming that Jupiter at an earlier period of his history performed a semi-revolution about his polar axis, and that all the inner satellites turned over, in like manner, in opposition to the orbital direction of their erratic outer member.

An insuperable objection to this ingenious hypothesis is the absence of any causal connexion between the assumed inversions of the axial motions of planets, together with their satellites, and their orbital revolutions, and, consequently, leaves untouched the problem of the retrograde orbital motion of a satellite, which it is the precise object of the hypothesis to explain. The fallacy involved in the scheme will at once be apparent when applied to the orbital rotations of all the planets which are clearly independent of the positions of their axes of rotation in relation to the plane of the ecliptic. And here it may be useful to apply Newton's 'First rule of reasoning in philosophy,' as laid down in the "Principia" that, 'we are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances; for Nature does nothing in vain, and more is in vain when less will serve, for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.'

I have already said that when a comet is ejected from a planet opposite to the orbital motion its direction would

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be retrograde to that of the planet from which it was ejected.

The orbital velocity of Jupiter being eight miles per second, a body ejected from its interior at a much greater velocity (which I will call the critical velocity) would, by the diminished attraction of the planet, conjointly with the action of solar gravity, revolve with a retrograde motion in an irregular and much enlarged orbit in accordance with the observations (*Plate* 1). And if ejected with a velocity much greater than that necessary to retain it within the sphere of the planet's attraction, the body would move in a separate and elliptical orbit as a comet.

Considering the comparative minuteness of Jupiter's three outer satellites, which are estimated to be less than thirty miles in diameter, and that the orbits of JVI and JVII are both inclined at 30° to the plane of the ecliptic, and have nearly the same periodic times and distances, these small bodies are hardly entitled to rank as satellites, but may rightly be regarded as planetary ejectamenta. Nevertheless, the discovery of them is of great importance, as furnishing another indirect proof of the planetary origin of comets.

Applying the foregoing principles of direct and retrograde motion of cometary bodies to the explosion of a whole planet between Mars and Jupiter, the fragments projected opposite to the orbital motion would be retarded, and by the action of solar gravity revolve in a smaller orbit than that of the planet before the explosion. On the other hand, the motion of the fragments coincident with the orbital direction would be increased, and by the diminished action of the sun's attraction, revolve in a larger orbit in accordance with the observations. In neither of these cases, however, would the orbits of the

fragmentary bodies again intersect each other at the point of the planet's orbit where the explosion occurred.

All the observations which I have made on the evolution of the Jovian satellites and cometary ejecta, are applicable alike to the Saturnian and other systems of planetary satellites. The evidence of orderly progression in the periodic times of the inner satellites of Saturn differs in one respect from that indicated by the satellites of Jupiter in similar positions, as the times of revolution of the first and third satellites are in the ratio of 1 and 2, and the times of the second and fourth are also in the same ratio, as was first pointed out by Sir John Herschel.*

Notwithstanding that the actual surface of Jupiter is covered with dense vapours of great depth, just as the terrestrial globe at one period of its history was enveloped with an atmosphere of aqueous vapour which has since condensed to form the oceans, several facts, in addition to those advanced indicate that the Jovian planet has a solid crust of considerable thickness.

The remarkably bright round spots which suddenly appear on the planet at irregular intervals, and have been described by Lassell, and also by Dawes, as having some resemblance to lunar craters,† indicate considerable volcanic activity below the atmospheric envelope. The eruptive matter from the Jovian craters also produces the appearance of belts on his outer surface as well as those seen on Saturn and Uranus. That these belts and bands are caused by volcanic dust ejected to great heights from the interior parts of planetary bodies is highly probable from observations made on the great eruption of Krakatoa in 1883.‡

* "Outlines of Astronomy," p. 368, 1864.

† *Monthly Notices Roy. Ast. Soc.*, vol. 10, 1850; *Ibid.*, vol. 18, 1857.

‡ "The Eruption of Krakatoa and Subsequent Phenomena." Report of the Krakatoa Committee of the Royal Society, 1888.

The ejecta from this volcano reached a height of more than 30 miles, forming a belt 20° wide on each side of the equator, and made two successive revolutions round the globe in the course of twenty-five days. The optical phenomena attending the eruption also included blue, green, and copper-coloured suns similar to the transient colours observed on the belts of Jupiter.

The problem of the origin of Saturn's rings has for a long time engaged the attention of natural philosophers, but no solution has yet been offered of sufficient importance to gain the general assent of astronomers. The first of these attempts was made in 1755 by Kant in his "Natural History and Theory of the Heavens," wherein he assumes that Saturn at an early period of its history had the characteristics of a comet and moved in an orbit of great eccentricity. That its tails gradually contracted upon the planet to form a cometic atmosphere of vapours which subsequently changed into the form of a ring entirely separated from the body of the planet.

In the "Système du Monde" of Laplace the rings are supposed to be the original nebular substance uncondensed into the form of satellites. This opinion has since been strongly held by astronomers and other scientific investigators and utilised as an illustration of the nebular theory of the origin of planetary systems.

Recent spectroscopic and mathematical investigations have, however, shown that the rings consist of a vast number of minute bodies, in confirmation of the views previously advanced by J. D. and J. Cassini in the *Memoirs of the French Academy of Sciences* in 1705 and 1715.

In neither of the explanations of the origin of Saturn's rings by Kant and Laplace is there any suggestion of the

interior of the planet as being the birthplace of these singular appendages. It is therefore with some amount of diffidence that I venture to affirm that they are the ejectamenta of Saturn when its diminishing energies were insufficient to eject a cometary satellite, or a comet with its train of meteorites beyond the sphere of its gravitational attraction. And here it may be well to remark that all meteoric and other small discrete bodies are not formed *directly* from the universal nebular substance, but are necessarily fragments of the solid or liquid parts of a globe, which had a long previous history, involving the evolution of the several series of elementary substances of which the globular body was composed.

The dimensions of Saturn's rings are drawn up in the following table for a new determination of the times of their revolutions, and are based upon the commonly accepted equatorial diameter of the planet = 73,860 miles or the semi-diameter of 36,930 miles.

The dimensions have been calculated from scaled measurements which I have made of reproductions of the fine photographs of Saturn taken at the Lick* and other Observatories during recent years, and which surpass in accuracy those calculated from observations and micrometric measurements.

The radial dimensions of the rings on the line of the equatorial diameter of the planet have the same proportional relations at different angles about this diameter, and constitute the basis of the method of measurements which I have adopted.

In accordance with the notation of O. Struve, now generally adopted, I have designated the rings A, B, and C, in the order of their distances from the planet.

* Todd, "Stars and Telescopes," 1900.

ELEMENTS OF SATURN'S RINGS.

	Distance from centre of Saturn.		Time of Revolution.	
Rings.	Sat. Units.	Miles.	h.	m.
Exterior A. ...	2·30	84,937	12	48
Breadth	0·26	9,602	,,	,,
Mid-breadth ...	2·17	80,138	11	45
Interior A. ...	2·04	75,337	10	42
Interval	0·07	2,585	,,	,,
Exterior B. ...	1·97	72,752	10	9
Breadth	0·47	17,357	,,	,,
Mid-breadth ...	1·735	64,073	8	24
Interior B. ...	1·50	55,395	6	44
Exterior C. ...	1·50	55,395	6	44
Breadth	0 23	8,493	,,	,,
Mid-breadth ...	1·385	51,148	6	00
Interior C. ...	1·27	46,901	5	15
Ball Space ...	0·27	9,971	,,	,,
Sat. Ball ...	1 00	36,930	10	13
Mimas	3·36	124,084	22	37

The velocity with which a body is ejected from the interior of a planet, as I have said, determines whether it shall be designated a comet, a cometary satellite, or a cometary ring. If the latter, it will be obvious that, from whatever part of the circumference of the planet the discharge takes place, the ejected matter will necessarily move in the same direction as the axial rotation. Moreover, if the discharge continued without interruption during one or more rotations of the planet a complete ring of discrete bodies would be formed in accordance with the accepted theory and observations.

It will be further evident from the three orders of cometary discharge specified above, the formation of the outer ring A preceded that of the next inner ring B, as shown by the interval of 2,585 miles of clear space between them.

That the second ring was formed some time subsequently to the first, is highly probable from the long period of intermittent discharges observable in terrestrial volcanoes, and also in celestial explosive action, of which there are abundant instances in planetary volcanoes and variable stars.

That the third and dusky ring C of Saturn represents its last and final effort of cometary evolution is shown by the wide separation of the discrete bodies of which the ring C is composed, and further indicated by its semi-transparency through which the body of the planet is distinctly visible.

I have not included in the table of distances the now well-recognized subdivisions of the exterior ring A and of the dusky ring C, so distinctly seen in the photographs, but they are sufficiently definite for a measurement to be taken of their width, which is approximately 230 miles.

The thickness of the rings is difficult to determine on account of the great distance of Saturn from the earth, and has been estimated by Herschel as not exceeding 250 miles. Assuming this value to be approximately correct, the vent in the crust of the planet through which the matter of the rings was ejected may not have been larger than those from which it is assumed the outer satellites of Saturn and Jupiter were also ejected.

The polar compression of Saturn is well determined by the photographic method when the edge of the ring alone is visible, and is in the ratio of 10 to 11 of the equatorial diameter. The value of the compression from good observations varies between $\frac{1}{9{\cdot}02}$ and $\frac{1}{10{\cdot}19}$.

Turning now to the times of revolution of Saturn's rings respecting which there are wide differences of opinion, arising from the fact that there are no distinctive marks on their surfaces from which their rotations can be determined.

Laplace and also Herschel were content to consider the rings as one body, and both assigned the period of its rotation to be 10 hours 32 minutes, as being the time of a satellite revolving at the same distance as the middle of its breadth.

Later investigators have, however, found it necessary to recognize, from the discrete constitution of the rings, the different times of revolution of their outer and inner circumferences, but have still treated them as one body, and assigned a period of 12 hours 5 minutes for the outer circumference, and 5 hours 50 minutes for the inner edge of the dusky ring C.

From the fact that the ring A is separated from the inner ring B by a clear space of 2,585 miles, the time of

its revolution may be determined independently of the times of B and C.

As the ring A is postulated to be the first annular ejection from the planet, its outer edge would be the extreme limit of the ejective force, and it would consequently revolve in the same time as a satellite at the same distance, in accordance with Kepler's third law. Now the period of Mimas, the first satellite of Saturn, is 22 hours 37 minutes, hence we have for the outer edge of the ring a periodic time of 12 hours 48 minutes; and 11 hours 45 minutes as the time of rotation at the middle of its breadth.

Dealing with the second ring B in the same manner, we have for the outer edge a period of 10 hours 9 minutes, and for the middle breadth, 8 hours 24 minutes as the period of revolution.

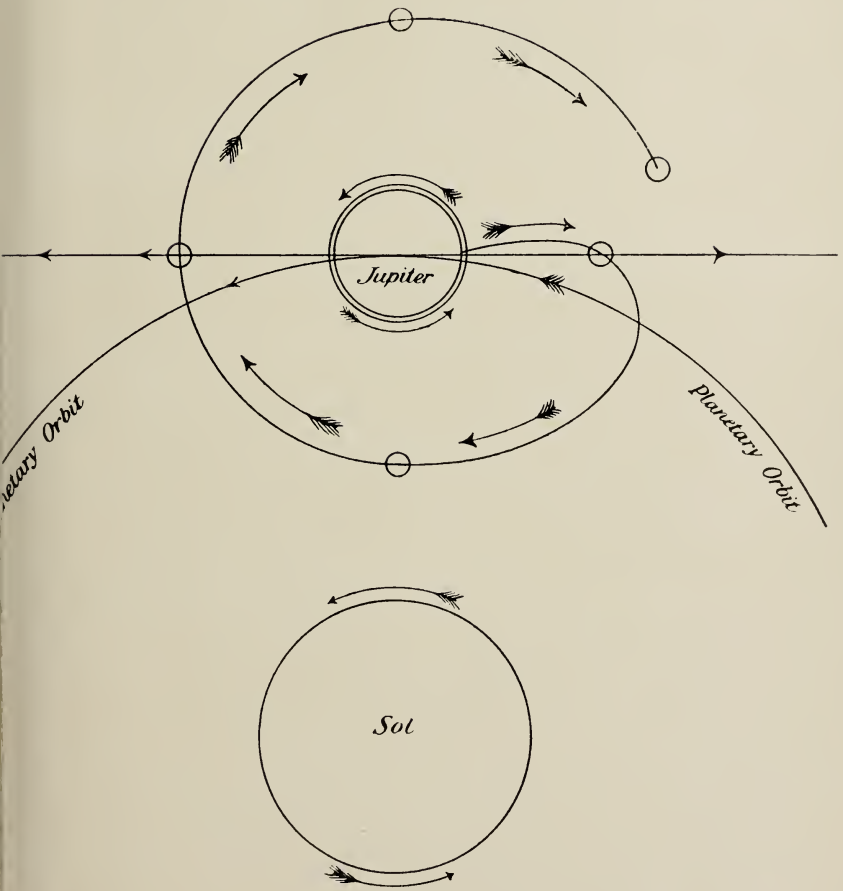
The determination of the time of revolution of the dusky crape ring C presents some difficulty on account of the wide separation of the discrete particles of which it is composed, and its apparently close contact with the interior of the ring B, but as by Kepler's law the time of revolution of the interior of B would be 6 hours 44 minutes, the exterior parts of C may be assumed to revolve at the same rate, and the inner edge of C in 5 hours 15 minutes.

From the principle of the transformation of energy it may be rightly inferred that some of the molar motion of the vast assemblage of discrete particles constituting the rings would be converted into heat, with a consequent slow contraction of their orbits. The observations collected by O. Struve in favour of such contraction have been discussed by astronomers, but without so far arriving at any definite conclusion.

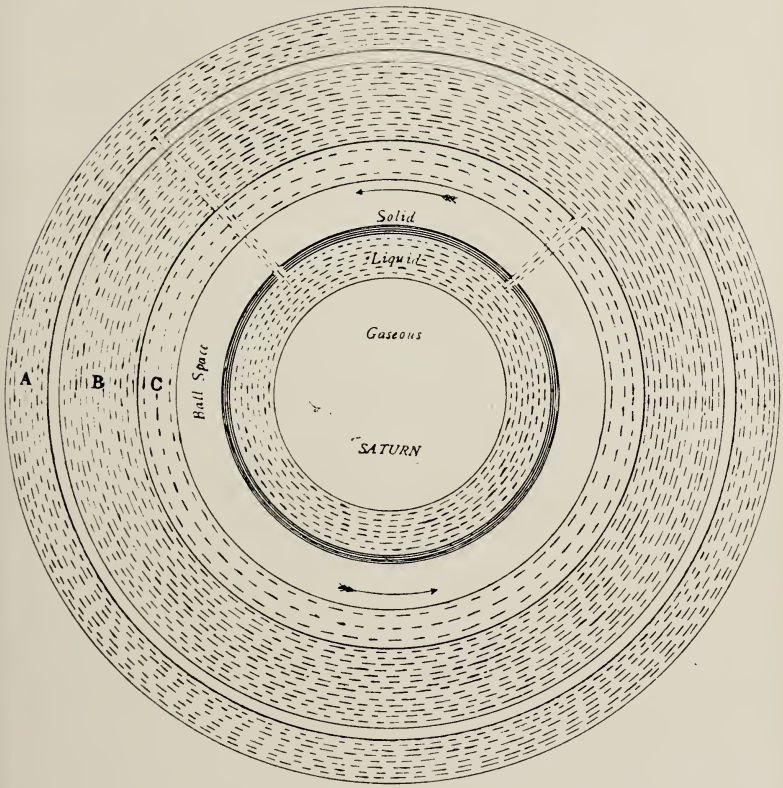
The resemblance of Saturn's rings to the Zodiacal Light is briefly indicated by Kant in a short chapter of his 'Theory of the Heavens,' in which he accounts for its origin by assuming that the fire of the sun raises from its surface vapours similar to those which formed Saturn's ring, and by their motion around the sun formed an expanded plain in the plane of the sun's equator, or in the figure of a convex lens.

Modern investigators have since carefully observed this singularly interesting object, and mostly agree that it is a vast accretion of cometary and meteoric particles from outer space and extending beyond the earth's orbit, but none of them, so far as I know, has suggested the interior of the sun as the place from which the Zodiacal substance has been ejected.

That cometary and meteoric matter may have contributed to the volume of discrete bodies surrounding the sun and extending to some distance within the orbit of Mercury has some degree of probability in its favour, but the extreme tenuity of the outermost parts of the Zodiacal substance, together with its immense distance from the central body, appears to me to be better accounted for on the supposition of its consisting of the lighter elementary substances in a state of extreme sub-division ejected during solar eruptions, as in the instance of the ejection of enormous masses of hydrogen observed by Young which I have already adduced.



COMETARY SATELLITES WITH RETROGRADE MOTION.



SCALED DIAGRAM OF SATURN'S RINGS.

Manchester Memoirs, Vol. lv. (1910), *No* 2.

II. Note on Scattering during Radio-active Recoil.

By Walter Makower, M.A., D.Sc.,

and

Sidney Russ, D.Sc.

Received and read November 15th, 1910.

In the course of some experiments on the recoil of radium B from radium A, it was found that not only did a surface directly exposed to the recoil stream become active, but surfaces situated outside the direct stream also received active deposit. It was thought that these effects were due to reflection or scattering from the surfaces

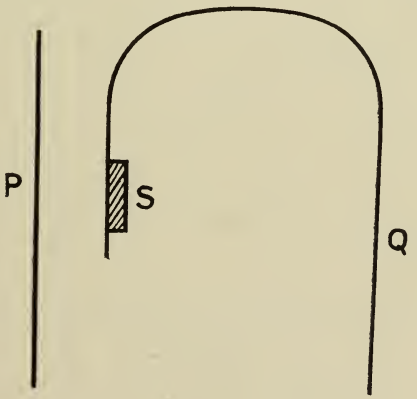


Fig. 1.

upon which the recoil-atoms fell, and a few preliminary experiments were made to test this hypothesis. The experiments, which were carried out in a high vacuum, were made in the following way. A plate S was mounted as shown in *Figure* 1 in such a way that it was outside

the recoil-stream coming from the active wire P, coated with radium A, but so that recoil-atoms reflected or scattered from the copper reflector Q could reach it. The distance from the wire to the reflector Q was 1·4 cms., and that from the reflector to the surface 1·2 cms. After an exposure of ten minutes in vacuo, the plate S was removed and found to be active, and the nature of the

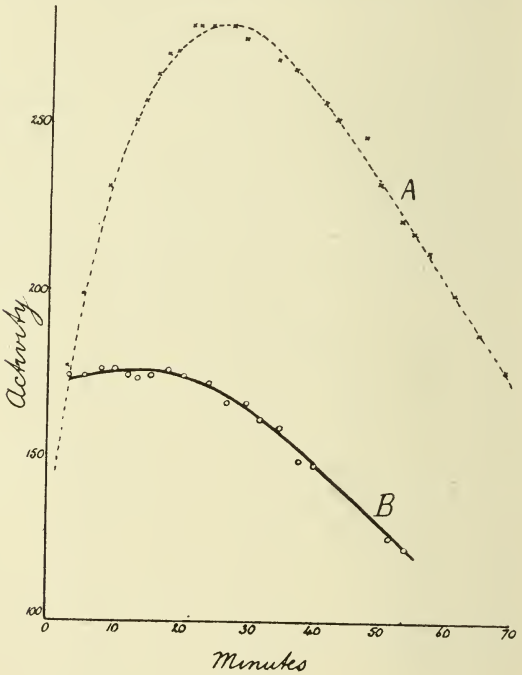


Fig. 2.

active matter on the plate was ascertained by measuring its rate of decay with an α-ray electroscope. After exposing a plate for ten minutes to the radium B expelled from the wire, the activity should at first rise and attain a maximum after 27 minutes, and then fall off with time as indicated by the dotted curve (*Figure* 2), which has been

calculated theoretically. It will be seen that curve B, which represents the results of the experiment just described, hardly rises at all, remaining nearly constant at first, and beginning to decay after about 20 minutes. The curve indicates that more than half of the active matter reaching S was radium C, and not radium B. This result can be explained if, when the radium B impinges on the reflector, a small portion of it is scattered on to S, but the greater part remains on the reflector, and subsequently gives rise to radium C, a small fraction of which is then directly projected on to the plate S. That the admixture of radium B with radium C on S is to be attributed to reflection is probable, since the matter reaching a surface by direct radiation from a wire coated with radium A consists only of radium B. An experiment was performed under these conditions, and the decay curve of the activity collected on a surface after an exposure of ten minutes to the radium A was obtained. The points lying on the curve A (*Figure* 2) were determined in this way, and we have seen that the curve itself was obtained by calculation for these experimental conditions. In an experiment in which a plate was situated so as to receive radium B from a source of radium A, only after a number of reflections, the proportion of radium C reaching the plate was even greater than in the case already cited. The fact that in the case of a single reflection considered above, radium B and radium C reached the plate S in almost equal proportions was a little surprising; for it has been shown that when a surface is coated with radium B, under normal conditions the number of atoms of radium C which succeed in escaping from the surface by recoil is only of the order of one thousandth of the total number formed.* The composition of the activity on the plate S

* Makower & Russ, *Phil. Mag.*, Jan., 1910.

can therefore only be explained either if the quantity of radium B reflected at Q is very small, or if the chances of recoil are greater under the present experimental conditions than in the previous experiments. The latter explanation seems to be the correct one, for we have seen that radium B and radium C reach the surface S in almost equal proportions, and the α-ray activity of the plate S was found to have about $\frac{1}{20}$ the activity of the surface Q when tested 20 minutes after the recoil from the wire P had ceased. Now it can be calculated from these facts that, if a small fraction x of the radium B recoil-atoms reaching Q are reflected on to the plate S, the fraction of radium C recoil-atoms subsequently reaching S to the total number formed on Q must be about $10x$. Taken in conjunction with the fact that the activity of Q was only twenty times that of S, this result leads to the conclusion that the proportion of radium C atoms which succeed in recoiling from the surface Q is greater than the fraction (one thousandth) previously obtained. Though it is not possible to be quite sure of this deduction from the above evidence, the conclusion is not unreasonable since the atoms of radium B deposited from the wire P by recoil are lightly distributed over the surface Q without any risk of being covered by surface films as might easily be the case with any other method of deposition. The whole question of the scattering of recoil-atoms is at present receiving more careful examination.

Manchester Memoirs, Vol. lv. (1910), *No.* 3.

III. The Development of the Atomic Theory: (2) The various Accounts of the Origin of Dalton's Theory.

By Andrew Norman Meldrum, D.Sc.

(Carnegie Research Fellow).

(Communicated by Professor H. B. Dixon, M.A., F.R.S.)

Received June, 1910. Read November 1st, 1910.

The origin of Dalton's theory remains one of the outstanding problems in the history of chemistry. Yet the amount of material at hand for the study of the subject is considerable. Dalton's note-books, discovered within the last twenty years in the rooms of the Manchester Literary and Philosophical Society, contain material of the highest value for the purpose. Also, there are on record important accounts of the genesis of the theory by three different persons. One is given by William Charles Henry, another by Thomas Thomson, and another by Dalton himself. Although there are yet other accounts in existence, these three are the only ones that need be considered in detail here.

One of the principal results of this paper is to show that these various narratives came, originally, from Dalton himself. In the nature of the case, this is what was to be expected. At the same time the discrepancies between these accounts have to be explained. In the course of the paper it will become more and more evident that the person responsible for them is Dalton.

December 17th, 1910.

1. *The Influence of J. B. Richter.*

William Charles Henry held a conversation with Dalton on the subject of the origin of the theory, in which special importance was given to the influence of J. B. Richter. "The speculations which gave birth to the atomic theory were first suggested to Mr. Dalton by the experiments of Richter on the neutral salts . . . a table was formed exhibiting the proportions of the acids and the alkaline bases constituting neutral salts. It immediately struck Mr. Dalton that if these saline compounds were constituted of an atom of acid and one of alkali, the tabular numbers would express the relative weights of the ultimate atoms. These views were confirmed and extended by a new discovery of Proust,"[1] &c.

This narrative received strong support from William Henry (the father of W. C. Henry), who held more than one conversation with Dalton on the subject. The following is part of a minute, dated February 13, 1830, of one of these conversations :—"Confirmed the account he before gave me of the origin of his speculations leading to the doctrine of simple multiples, and of the influence of Richter's table in exciting these views."[2]

The Henrys, father and son, are entitled to the fullest credence in this matter. Their acquaintance with Dalton was more intimate than that of any other man of science, Peter Clare excepted. W. C. Henry was in turn the pupil, the friend and the biographer of Dalton. In the preface to the Biography, he mentions with just pride Dalton's "almost lifelong friendship with my father, never shadowed by even a passing cloud"; and he refers also to "his early favourable notice of and unceasing benevolent regard towards myself, thoughtfully mani-

[1] W. C. Henry, "Memoirs of Dalton," p. 84.

[2] *Ibid.*, p. 63.

fested in his last bequest to me of what he had most prized in life." This was the bequest of all his chemical and philosophical instruments and apparatus. Other proofs of this friendship can easily be found. There is the dedication of Dalton's "New System of Chemical Philosophy" (vol. i., Part 2) to William Henry (along with Humphrey Davy), and of Henry's "Elements of Experimental Chemistry" (6th Ed., 1810) to Dalton. Again, Dalton took an opportunity in 1827 of acknowledging his friendship with William Henry. "It affords me great pleasure to acknowledge the continued and friendly intercourse with Dr. Henry, whose discussions on scientific subjects are always instructive, and whose stores are always open when the promotion of science is the object."[3]

There is no room for doubt that the reports of these conversations with Dalton are perfectly authentic. W. C. Henry states that he noted down Dalton's expressions "immediately after each lesson," and the passage which has been quoted, regarding the influence of Richter, is copied, he says, "verbatim from my own journal when his pupil."[4] Nevertheless, Henry knew there was something wrong. The date of his conversation with Dalton was February 5, 1824, and he says, "on reviewing in conversation, after the lapse of twenty years, the labours of the past, Dalton himself may have failed in recalling the antecedents of his great discovery in the exact order of sequence"[5]

Again, the Richter story is strongly challenged by Thomas Thomson. "When I visited him in 1804 at Manchester both Mr. Dalton and myself were ignorant of

[3] "New System of Chemical Philosophy," vol. 2, p. 8, 1827.

[4] *Ibid.*, p. 84.

[5] *Ibid.*, p. 86.

what had been done by Richter on the same subject." Again, "Nobody knows better than myself that Dalton was ignorant of what Richter had done about ten years before him."[6] This shows conclusively that Dalton said nothing about Richter to Thomson.

Now that we have access, thanks to Roscoe and Harden's "New View of the Origin of Dalton's Atomic Theory," to the valuable material contained in Dalton's notebooks, we can carry the critical process further than Henry and Thomson did. The notebooks show, as Roscoe and Harden point out, that Dalton had been busily engaged during the year 1803 on the atomic theory, and that he was investigating the non-metallic elements then, and not Richter's acids and bases at all.

Dalton's knowledge of Richter can hardly have been due to anyone but Berthollet. Richter's work had been completely ignored till E. G. Fischer gave a resumé of it, and thus made it known throughout Germany. Berthollet, by quoting this resumé at the end of the "Essai de Chimie Statique," made Richter known throughout Europe. In the "Essai" Berthollet opposes Dalton's theory of "mixed gases," but Dalton made no reply till 1808 in the "New System of Chemical Philosophy." This helps to date his knowledge of Richter. If Dalton was slow to read new books, he was prompt in replying to criticisms of his theory. He kept up the defence of it in a series of papers which came to an end about October, 1805, without any mention of Berthollet's objections having been made. It was presumably subsequent to this date that Dalton read the "Essai," and learnt of Richter's work. In the note-books the date of the earliest reference to Richter is April 19th, 1807.[7] There is really no room for doubt that

[6] *Proc. Phil. Soc. Glasgow*, vol. 2, pp. 86, 88, 1845-6.

[7] Roscoe and Harden, "New View of the Origin of Dalton's Atomic Theory," p. 79; see also pp. 7-10, 46, 91-94.

Dalton's declarations in 1824 and 1830 to one and the same effect regarding the influence of Richter must be set aside.[8]

2. *The Composition of Marsh-gas and Olefiant Gas.*

Thomas Thomson says that the theory first occurred to Dalton during his investigation of marsh-gas and olefiant gas. The discovery of the composition of these gases led to the discovery of the law of multiple proportion, and the theory was then devised in order to explain the law. His exact words are :—

"Mr. Dalton informed me that the atomic theory first occurred to him during his investigations of olefiant gas and carburetted hydrogen gas, at that time imperfectly understood, and the constitution of which was first fully developed by Mr. Dalton himself. It was obvious from the experiments which he made upon them that the constituents of both were carbon and hydrogen, and nothing else. He found, further, that if we reckon the carbon in each the same, then carburetted hydrogen contains exactly twice as much hydrogen as olefiant gas does. This determined him to state the ratios of these constituents in numbers, and to consider the olefiant gas a compound of one atom of carbon and one atom of hydrogen ; and carburetted hydrogen of one atom of carbon and two atoms of hydrogen. The idea thus conceived was applied to carbonic oxide, water, ammonia, &c., and numbers were given representing the atomic weights of oxygen, azote, &c., deduced from the best analytical experiments which chemistry then possessed."[9]

This narrative has passed muster for many years, and is better known than any other. It was accepted with

[8] Roscoe and Harden, *loc. cit.*

[9] Thomas Thomson, "History of Chemistry," vol. 2, p. 291.

reservations by W. C. Henry[10] and Angus Smith[11], and by Roscoe and Schorlemmer[12] without objection. Owing to the large circulation of Roscoe and Schorlemmer's book, this version of the origin has decided the opinion of the generality of chemists. There is, nevertheless, the best reason for thinking that marsh-gas and olefiant gas did not have the effect which it assigns to them of leading to the theory.

Indeed, in 1811, Dalton connected the theory in its early days with the oxides of nitrogen:—"I remember the strong impression which at a very early period of these inquiries was made by observing the proportion of oxygen to azote, as 1, 2, and 3, in nitrous oxide, nitrous gas, and nitric acid, according to the experiments of Davy."[13] Thomson must have seen the necessity of abandoning the marsh-gas and olefiant gas story, for he said in 1850:—"Dalton founded his theory on the analysis of two gases, namely, protoxide and deutoxide of azote."[14]

Dalton's work on marsh-gas appears in the note-book under date 6th August, 1804. Roscoe and Harden[15] point out that he had been busily engaged on the theory the year before. He had even arrived at the fundamental ideas of his system, and had constructed a table of atomic weights by September 6th, 1803.

Obviously, Thomson's account of the origin of the theory is untrustworthy, inasmuch as marsh-gas and olefiant gas had no part in the matter. The question arises, who is responsible for the error, Thomson or Dalton? Before answering this question it is necessary

[10] "Memoirs of Dalton," p. 80.

[11] "Memoir of Dalton," p. 231.

[12] "Treatise on Chemistry, Non-metallic Elements," p. 36, 1877.

[13] *Nicholson's Journ.*, vol. 29, p. 143, 1811.

[14] *Proc. Phil. Soc. Glasgow*, vol. 3, p. 140, 1850.

[15] *Op. cit.*, p. 28.

to consider carefully the relations between the two men and the circumstances under which Thomson's narrative arose.

Thomson, unlike the Henrys, was not a personal friend of Dalton. He had made an adverse criticism of a certain theory of which Dalton was the author, and the author had made a stiff rejoinder.[16] He thereupon paid a visit to Manchester with the object of arriving at a full understanding of the matter in question. The date of the interview was August 27th, 1804, and it was then, by a fortunate accident, that Thomson learnt of the chemical atomic theory of Dalton.

Again, it is certain that Thomson and Dalton were not subsequently in frequent communication with one another on the subject. The sketch of the theory, which Thomson published in 1807, was accompanied by the note :—" In justice to Mr. Dalton, I must warn the reader not to decide upon the notions of that philosopher from the sketch which I have given, derived from a few minutes conversation, and from a short written memorandum. The mistakes, if any occur, are to be laid to my account, and not to his ; as it is extremely probable that I may have misconceived his meaning in some points."[17]

Nevertheless, this footnote errs on the side of caution. Thomson's sketch of the theory, giving the first account of it ever printed, was based on notes of what Dalton told him, made during the interview, and only one phrase in it is open to objection. He showed both zeal and care in the matter, for it strongly interested him.

In the "History of Chemistry," published in 1831, Thomson says :—" I wrote down at the time the opinions which he offered, and the following account is taken

[16] See *Nicholson's Journ.*, vol. 8, p. 145, 1804 ; and *Annals of Philosophy*, vol. 4, p. 65, 1814.

[17] Thomas Thomson, "System of Chemistry," 3rd Ed., vol. 3, p. 425, 1807.

literally from my journal of that date."[18] Then comes an account of the atomic theory, and on that there follows the passage already quoted, connecting marsh-gas and olefiant gas with the genesis of the theory. Here the question arises, is all this taken from the journal, both the sketch of the theory and of how the theory arose? Only an examination of the journal can settle this point, but I have not succeeded in ascertaining where it is kept, if, indeed, it is still in existence.

It must be admitted also that Thomson seems to become more and more positive regarding the genesis of the theory as time goes on. The account which I have been considering was published in 1831. Six years earlier he had advanced the same account in a more hesitating way:—"Unless my recollection fails me, Mr. Dalton's theory was originally deduced from his experiments on olefiant gas and carburetted hydrogen."[19] Yet there is no intrinsic improbability that Thomson's recollection is correct. One cannot doubt that during the interview Dalton was much less interested in the question of the origin than in the theory itself. If Thomson inquired about the origin, Dalton may have made the inquiry an opportunity of expounding the theory in terms of its latest triumph, namely, the composition of marsh-gas and olefiant gas.

3. *The Amended Theory of "Mixed Gases."*

There remains for consideration the account which Dalton gave in a lecture (the 17th of a series) at the Royal Institution of London, on the 27th January, 1810. The

[18] Thomas Thomson, "History of Chemistry," vol. 2, p. 287.

[19] Thomas Thomson, "An Attempt to Establish the First Principles of Chemistry by Experiment," vol. 1, p. 11, 1825.

notes for it still exist in his own handwriting, and were found, along with his notebooks, in the rooms of the Manchester Literary and Philosophical Society. He begins by discussing his physical atomic theory, which aimed at explaining the diffusion of gases. He entertained two diffusion hypotheses, the first of which originated in 1801, while an amended hypothesis, he says, was formed in the year 1805. He had not at first "contemplated the effect of *difference of size* in the particles of elastic fluids." On consideration, he "found that the *sizes must* be different," and subsequently arrived at a different explanation of the mechanism of diffusion from the one he at first suggested.

He then introduces the subject of the chemical atomic theory:—"The different *sizes* of the particles of elastic fluids under like circumstances of temperature and pressure being once established, it became an object to determine the relative *sizes* and *weights*, together with the relative *number* of atoms in a given volume. This led the way to the combination of gases . . . other bodies besides elastic fluids, namely, liquids and solids, were subject to investigation, in consequence of their combining with elastic fluids. Thus a train of investigation was laid for determining the *number* and *weight* of all chemical elementary principles which enter into any sort of combination one with another."[20]

This narrative is certainly right on a vital matter. It recognises that Dalton had been using a physical atomic theory, from which he passed to a chemical one. Here there is a common ground of objection to the communications made by Dalton to Thomson and Henry respectively. They both ignore the connection, which certainly existed, between the physical and chemical theories. Thomson did not feel this defect, but Henry

[20] Roscoe and Harden, *Op. cit.*, pp. 16—17.

did. While not denying the influence of Richter, he sums up the evidence on the subject as "unequivocally demonstrating the genesis of the atomic theory as a general physical conception from the study of matter in the *aëriform* condition, and its first practical application in chemistry to *gaseous* bodies, and emphatically to such as combine *in multiple proportions.*"[21] There is no question here of extraordinary insight and discernment on Henry's part. He has simply considered the use Dalton had made of the physical atomic theory previous to forming a chemical one.

Roscoe and Harden have not paid sufficient attention to this. They say "It is . . . . well known that Dalton was an ardent adherent of the Newtonian doctrine of the atomic constitution of matter . . . . It now appears that it was from this physical standpoint that Dalton approached the atomic theory, and that he arrived at the idea that the atoms of different substances have different weights from purely physical considerations."[22] There is really not sufficient justification for Roscoe and Harden's suggestion that they had found in Dalton's narrative a *new* view of the genesis of his atomic theory. The view is to be found in Henry, and might be formed by any person who should read with understanding Dalton's "Essay on the Constitution of Mixed Gases," which was written in 1801, and published in 1802.

There is, however, a fundamental objection to Dalton's narrative. It has a deceptive appearance of being historical. Dalton was a pioneer of science, and a pioneer is a man who must make many mistakes and experience many failures. He has taken a number of different scientific movements and marshalled them, so that they are invested

[21] W. C. Henry, *Op. cit.*, p. 84.

[22] Roscoe and Harden, *Op. cit.*, p. viii.

with the appearance of a deliberate, strategical, irresistible advance. On examination his narrative, in spite of its grand air, is found to throw much less light than it promises on the line of thought and train of investigation which he pursued. It is excessively abstract in tone, and avoids going into details and particulars and instances. It does not tell us what we want to know most, how and when Dalton arrived at the law of multiple proportions, and the part played by the law in the construction of the theory. Information on these matters is what is wanted, and anything else is beside the point.

Yet there is one novel element in Dalton's account. This is the suggestion that the formation of the chemical atomic theory took place subsequently to the amendment of the diffusion theory. But, as the notebooks show, the chemical theory was formed in 1803. Hence, Roscoe and Harden conclude that 1805, the date which Dalton assigns to his amended diffusion theory, should be 1803.[23] Reasons will be given later, in a paper on Dalton's physical atomic theory, for thinking that the narrative is doubtful on the only point on which it presents any novelty.

Conclusion.

There are in existence yet other accounts of this matter. One is given by Dalton's pupil, Joseph A. Ransome,[24] and another by Dalton himself. This was in the lecture which he delivered to the members of the Mechanics' Institute in Manchester on October 19th, 1835.[25] The main feature, which *all* the accounts have in

[23] *Op. cit.*, p. 25.

[24] W. C. Henry, *Op. cit.*, pp. 220-222.

[25] *Manchester Times*, October 25, 1835.

common, is that each originated with Dalton. Thomson's narrative and Henry's and Ransome's were based on conversations with him, and there is no ground for impugning their accuracy any more than his good faith. The natural explanation of the existence of so many and various accounts is that Dalton was simply deficient in historical instinct. He did not perceive the difference between describing the genesis of his theory and expounding the theory itself.

A man who makes history, as Dalton did, need not be a good historian. The account of the origin of the chemical theory in his own handwriting is no more satisfactory than the others which came from him at second-hand. Apparently, Dalton never had in his mind a precise view of how the theory developed, and when invited to give one he produced, on the spur of the moment, an account to which he did, or did not, adhere on the next occasion.

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IV. The Development of the Atomic Theory: (3) Newton's Theory, and its Influence in the Eighteenth Century.

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One of the great obstacles to a right understanding of the history of science, is the tendency of writers to let their attention be absorbed by a single individual, who thus engrosses the credit for important ideas and discoveries, to the neglect of deserving predecessors. This method, besides being unjust, gives a distorted view of the progress of science. For instance, Nernst, apropos of Dalton, remarks that the atomic hypothesis "by one effort of modern science, arose like a phœnix from the ashes of the old Greek philosophy"[1] This sweeping statement ignores atomic speculation between the time of Lucretius and the nineteenth century. As if the atomic theory of Newton, for instance, were perfectly negligible!

This paper is written in the belief that the atomic theory has gone through a process of development from the time of Leucippus up to the present. The main conclusions are that Newton made a contribution to the said process, that he did so under the influence of Descartes, and that he was, in turn, himself an influence in the eighteenth century. It is therefore divided into two parts: (1) The atomic theory of Newton, and (2) Newton's influence in the eighteenth century.

[1] Nernst, "Theoretische Chemie," 5th ed., p. 34.

December 17th, 1910.

I. *The Atomic Theory of Newton.*

In the seventeenth century the atomic theory is associated with the famous names of Francis Bacon (1561—1626), René Descartes (1596—1650), Pierre Gassend (1592—1655), Robert Boyle (1627—1691) and Isaac Newton (1642—1727).

Bacon recurs to the theory again and again in his philosophical writings, as if fascinated by it. At one time he entertained great expectations from the study of the atoms. "I know not whether this inquiry I speak of concerning the first condition of seeds or atoms be not the most useful of all, as being the supreme rule of art and power, and the true moderator of hopes and works."[2] This in the "Cogitationes de Natura Rerum," which is regarded as having been composed before the year 1605. But he changed his mind on the subject, tending, as time passed, to become more and more distrustful of *a priori* reasoning. His mature judgment, as expressed in the "Novum Organum," published in 1620, was that the atoms are an unprofitable study. "Men cease not . . . from dissecting nature till they reach the atom; things which, even if true, can do but little for the welfare of mankind."[3]

Boyle, in this country, was the exponent of the atomic theory who brought it into repute. In the year 1659 he urged the "desirableness of a good intelligence between the Corpuscularian Philosophers and the chemists,"[4] and this topic for some time afterwards he made a leading theme in his scientific writings. Within a few years of his first attempt he was able to say that he has "had the happiness

[2] Bacon's Works, ed. by Spedding & Ellis, vol. 5, p. 423.

[3] *Op. cit.*, vol. 4, p. 68; or *Nov. Org.*, 1, aphorism 66.

[4] Boyle's Works, ed. by Birch, vol. 1, p. 227, 1744.

to engage both divers chymists to learn and relish the notions of the Corpuscular Philosophy, and divers eminent embracers of that to endeavour to illustrate and promote the new philosophy by addicting themselves to the experiments and perusing the books of chemists."[5] While on this subject, he mentions Descartes and Gassend constantly, and other philosophers hardly ever.

Descartes believed in the existence of atoms, and at the same time he denied that a void could exist. A subtle fluid occupied the space between the atoms, and even permeated them. Hence the vortex motion which had been set up in the fluid could not but communicate itself to the atoms. An admirable description of the atmosphere, according to the Cartesian theory, is to be found in Boyle's "New Experiments, Physico-Mechanical, touching the Spring of the Air." "The restless agitation of that celestial matter, wherein these particles [of air] swim, so whirls them round, that each corpuscle endeavours to beat off all others from coming within the little sphere requisite to its motion about its own centre . . . their elastical power is made to depend . . . upon the vehement agitation . . . which they receive from the fluid ether that swiftly flows between them."[6] It is remarkably difficult to find in Descartes so good a description of his theory as this.[7]

Descartes' denial that a vacuum could exist, it is plain from this, is not to be taken in the crudest sense. He never meant and never said that space is full of matter of the ponderable kind.[8] He meant, surely, that in the

[5] *Op. cit.*, vol. 2, p. 501.

[6] *Op. cit.*, vol. 1, p. 8.

[7] Œuvres, ed. by Cousin, vol. 5, p. 159-162, 169-170.

[8] Clerk Maxwell might well have emphasised this in his comment on "The Error of Descartes," in "Matter and Motion," article xvi.

absence of ponderable matter, space is occupied by ether, "the celestial matter;" in short, that "we have no means of producing an ether-vacuum."

A more conventional theory is due to the revival, by Gassend, of the Epicurean philosophy. His interest in this philosophy was such that for twenty years he devoted himself to the study of Epicurus, and Lucretius the Epicurean.[9] Gassend sought to connect the atomic theory with both physical and ethical problems, for those were the days when natural and moral philosophy were studied by the same persons. He brought out three books on the subject, between the years 1647 and 1649, one of which, the "Syntagma Philosophiae Epicuri," was well known to Boyle.

Boyle learnt of the work through his friend Samuel Hartlib, who wrote to him, in a letter dated London, May 9th, 1648: "Your worthy friend and mine, Mr. Gassend, is reasonable well, and hath printed a book of the life and manners of *Epicurus*, since your going from here. He hath now in the press at Lyons the philosophy of *Epicurus*, in which, I believe, we shall have much of his own philosophy, which doubtless will be an excellent work." [10]

There was then, as there is still, a tendency to regard Descartes and Gassend as opponents of one another on the principles of the atomic theory. Boyle mentions some "learned men as more favouring the Epicurean, and others (though but a few) being more inclinable to the Cartesian opinions." However, in one of his essays, he advises *Pyrophilus* to read the "learned *Gassendus*, his

[9] For a study of the Lucretian philosophy, see "Lucretius, Epicurean and Poet," 2 vols., by John Masson. Chap. 1, vol. 2, is devoted to Gassend, of whom it gives a most interesting account.

[10] Boyle's Works, ed. by Birch, vol. 5, p. 257, 1744.

little *Syntagma* of *Epicurus*' philosophy, and that most ingenious gentleman, Mons. *Descartes*, his principles of philosophy."[11] He did not see any necessity to ally himself with one party or the other. "Notwithstanding those things, wherein the atomists and the Cartesians differed, they might be thought to agree in the main, and their hypotheses might, by a person of a reconciling disposition, be looked on . . . as one philosophy."[12]

Science has often gained immensely by a wise limitation of the problem to be solved. Descartes' theory, that space is pervaded by an ethereal fluid, and that ordinary matter consists of atoms swimming in the ether, is formally complete, and has to be adopted sooner or later. Yet Gassend's theory, which is incomplete, since it ignores the ether, and concentrates attention on the atoms, proved more helpful to science in the first instance. Newton was more inclined to Gassend's way of thinking than to Descartes'. In the "Principia" he would not consider the mechanism of gravitation, and in the course of his atomic speculations he almost leaves out of account the means by which chemical attraction arises. Nevertheless, Newton was influenced by Descartes.

The Cartesian natural philosophy was predominant throughout Europe for the most part of the seventeenth century, and, in the eighteenth, it was supplanted by the Newtonian philosophy, as expounded in the "Principia." The two philosophies being opposed to one another, no one apparently has reflected how much Newton may have been indebted to Descartes. The mere fact that Cartesianism was dominant during the seventeenth century means that Newton must have made himself master of that system of nature. Presumably

[11] *Op. cit.*, vol. **1**, p. 194.

[12] *Op. cit.*, vol. **1**, p. 227-228.

then, whatever was sound in Descartes he retained and assimilated. Boyle and Hooke had studied Descartes, and Newton studied all three. In a letter to Hooke, dated Feb. 5th, 1675/6, on the subject of light, he admits his indebtedness to others. "You defer too much to my ability in searching into this subject. What Descartes did was a good step. You have added much several ways, and especially in considering the colours of thin plates. If I have seen further, it is by standing on the shoulders of giants."[13]

Newton, in his speculations on the disintegration of atoms, in Query 31 of the "Optics," had no unusual physical phenomenon in view at the time. He was simply improving on Descartes,[14] whose theory on the subject seems crude enough.[15]

In contrast to the speculative topic of disintegration, another problem which interested Newton was a perfectly concrete one. This was Boyle's law, made known in the year 1662, that the volume of a given quantity of air is inversely proportional to the pressure. Newton's theory of gravitation was based on the assumption that every particle of matter attracts every other particle. In explaining Boyle's law he made the very different assumption that air is composed of particles which repel one another.

This conception of the atmosphere, as being composed of "particles mutually repulsive," was in all probability derived from Descartes. Boyle, in the passage already quoted, where he explains the Cartesian theory, says that in the air, "each corpuscle endeavours to beat off all others from coming within the little sphere requisite to its motion about its own centre."

[13] Brewster's "Life of Newton," vol. 1, p. 142.

[14] Œuvres, ed. by Cousin, vol. 4, pp. 266-268.

[15] I am indebted to my friend, Mr. J. R. Partington, B.Sc., for pointing out to me that Descartes was the source of Newton's ideas on disintegration.

Newton proved that the air must obey Boyle's law, if the force of repulsion between its particles were inversely proportional to the distance between them. He does not mention Boyle, or the air, but puts the matter in the most abstract way, by advancing the following proposition :—"If the density of a fluid which is made up of mutually repulsive particles, is proportional to the pressure, the forces between the particles are reciprocally proportional to the distance between their centres. And *vice versa*, mutually repulsive particles, the forces between which are reciprocally proportional to the distance between their centres, will make up an elastic fluid, the density of which is proportional to the pressure."[16]

Newton does not draw any inference as to the nature of the atmosphere. "All these things are to be understood of particles whose centrifugal forces terminate in those particles that are next them, or are diffused not much further. We have an example of this in magnetical bodies. . . . . Whether elastic fluids do really consist of particles so repelling each other, is a physical question. We have here demonstrated mathematically the property of fluids consisting of particles of this kind, that hence philosophers may take occasion to discuss that question."

This proposition, along with its proof in the "Principia," is the earliest instance of the mathematical treatment of the atomic theory. Svante Arrhenius declares that "the atomic theory remained in the hypothetical state for about 2,300 years, as no quantitative conclusions were drawn from it till the time of Dalton."[17] This statement entirely ignores Newton's explanation of Boyle's law in terms of atoms, as well as certain workers in the eighteenth century, who were under Newton's influence.

[16] "Principia," Book 2, prop. 23.

[17] Arrhenius, "Theories of Chemistry, Eng. trans., p. 15.

II. *Newton's Influence in the Eighteenth Century.*

In the last quarter of the eighteenth century a very remarkable attempt at an atomic theory was made by two Irishmen, by name Bryan Higgins and William Higgins. The object of the second and concluding part of this paper is to show that the theory advanced by Bryan Higgins and amplified by William Higgins can be understood only when regarded as springing, under the peculiar conditions of the time, from Newton's theory. These conditions were (1) the knowledge, due to Priestley, of different kinds of gases, and (2) the new light which Lavoisier threw on chemical composition consequent on Priestley's discovery of oxygen.

The senior of the two men,[18] Bryan Higgins (1737-1820) was self-taught in chemistry, and his career proves him to have been the best all-round man among the English-speaking chemists of his day. His "Experiments and Observations concerning Acetous Acid" (1786) is a record of a very thorough investigation in the field of organic chemistry, in the course of which he discovered the substance acetamide. As a technical chemist his reputation was wide. He spent about four years (1797-1802) in the West Indies, investigating the manufacture of Muscovado sugar and rum. He was a pioneer in the practical teaching of chemistry, and gave instruction in the subject for some twenty-three years (1774-1797) in his *School of Practical Chemistry* in Greek Street, Soho, London. His minor discoveries include that of the musical note which can be got on burning a jet of hydrogen in air (1777).

[18] For fuller information regarding them, see *Brit. Assoc. Rep.*, Dublin meeting, 1908, p. 668, and *New Ireland Review*, 1910, n.s., vol. **32**, pp. and 350-364.

His most important publication, in connection with the atomic theory, is a "Philosophical Essay Concerning Light" (1776). This essay is very different from what it purports to be. It contains only a fragment—all that was ever published—of the essay on light that Higgins had designed. The major part of the book is simply an expansion and exposition of a "Syllabus of Chemistry," which he had published earlier, in 1774 or 1775, and which is also prefixed to the Essay.

Higgins had gone to Newton for inspiration: the "Philosophical Essay" is full of quotations from the "Opticks." Nor need there be any wonder at Higgins making his approach to the study of light by way of chemistry, since Newton's views on chemical subjects are to be found in the "Opticks" more than in any other of his books.

The discovery of new facts always gives a stimulus to speculation. The impulse in Higgins' case came from Joseph Priestley, who showed in the year 1775 that the alkaline substance ammonia, and various acids, hydrochloric, for instance, can exist in the gaseous state. Higgins thereupon proceeded to adapt the Newtonian conception of a gas to the processes of chemistry. Gaseous particles of the same kind were "mutually repulsive," but what should happen in case acid and alkali were brought together? Higgins said that the acid particles and the alkaline attracted one another, and formed a neutral salt by combining *particle with particle.*

Higgins laid great stress on this force of repulsion between particles of the same kind. He thought an acid and an alkali must combine with one another in one proportion only, a combination of two particles of acid and one of alkali, or two of alkali and one of acid, being precluded, because the two similar particles

must repel one another. On this line of thought he finds the answer to his own question:—"Why do many salts crystallise nearly neutral in a liquor containing a superabundant quantity of acid and [*sic*] of alkali?" Further, on the supposition that particles of water and of acid attract one another, as also particles of water and of alkali, he thought he could account for the water of crystallisation found in many salts, so he explains "why much water doth combine in the crystals of most neutral salts, and why this water of crystallisation separates from the superfluous acid or alkali, and introduces little or none of either into the crystals." [19]

In short, on the basis of Newton's theory of a gas, Bryan Higgins taught that chemical combination takes place between acid and alkali in a definite and single proportion. He went little further, if any, with these speculations. His progress must have been greatly hampered by his belief, to which he adhered till about the year 1792, in the phlogiston theory of chemistry, and by his belief in the existence of seven chemical elements, namely, earth, water, air, acid, alkali, phlogiston and light.

William Higgins (1769?-1825) was trained in chemistry by his uncle. He assisted Dr. Beddoes in the teaching of chemistry at Oxford (1787), and acted as chemist to the Apothecaries' Hall of Ireland (1791-1795), and then to the Royal Dublin Society (1795-1825). He was a Fellow of the Royal Irish Academy and of the Royal Society of London.

He did not long suffer from the disadvantages of the phlogiston theory, for he was one of the first to

[19] Bryan Higgins, "A Philosophical essay concerning Light," pp. 201-208, 212-213.

abandon it—in 1785, he says—and was absolutely the first to write against it in the English language. His "Comparative View of the Phlogistic and Anti-phlogistic Hypotheses" (1789) is primarily a refutation of the phlogiston theory. Incidentally, it shows that he had been carrying on experimental work of his own, and also that he had improved on his uncle's speculations. Out of atoms and molecules he fashioned a theory of chemical combination and chemical dynamics as well, so that his book is remarkable as containing the first attempt at a comprehensive system of chemistry, based on the atomic theory.

William Higgins regarded the atom of a gas as a hard particle surrounded by an "atmosphere of fire."[20] He believed firmly that chemical combination occurs in definite proportions, and supposed that it occurs, in the first place, atom with atom. He regarded the molecule of water as formed by the linking of one atom of hydrogen with one of oxygen. "Water is composed of molicules formed by the union of a single ultimate particle of dephlogisticated air to an ultimate particle of light inflammable air . . . they are incapable of uniting to a third particle of either of their constituent particles."[21] In short the formula OH expresses his conception of the molecule of water.

William Higgins was better acquainted with the facts of chemical composition than his uncle, for he did not believe in phlogiston, and he recognised oxygen as one of the elements. He was aware of a number of cases in which elements combine in more than one proportion, and in such cases continued to apply the atomic theory.

[20] "Comparative View of the Phlogistic and Antiphlogistic Hypotheses," pp. 14, 37, 81, 133.

[21] *Ibid.*, p. 37.

He thought an element R must form oxides in the order RO, RO_2, RO_3, etc. Thus he regarded sulphurous acid virtually as SO, and sulphuric acid as SO_2.[22] He recognised five oxides of nitrogen, and regarded them as NO, NO_2, NO_3, NO_4, and NO_5.[23] These ideas of chemical composition are based on the assumption that similar atoms repel one another, an assumption which is also the basis of his system of chemical dynamics. His argument was that because of this force of repulsion, the compound RO is more stable than RO_2, RO_2 than RO_3, and so on.

The line of thought thus opened by the Higginses afterwards proved extremely valuable, but it was not followed up at the time. William Higgins' book, published in 1789, and re-published in 1791, was read as a contribution to the phlogiston and anti-phlogiston controversy. That was the absorbing topic in science then, and nothing else could be duly attended to.

The history of this eighteenth century movement proved a difficult problem in the succeeding century. It occupied the attention at different times of such persons as William Charles Henry, R. Angus Smith, and, in collaboration, Roscoe and Schorlemmer. There was also a long and doubtful controversy regarding the relative merits of William Higgins and John Dalton, the discussion of which is left to a future paper.

Angus Smith's estimate of Bryan Higgins is a vastly different one from that advanced in this paper. His main conclusions are, that Bryan Higgins' "opinions on atoms might have been held by the ancients,"[24] and "that his theory was not clear, or he would have been led by it to

[22] *Ibid.*, pp. 36-37.

[23] *Ibid.*, pp. 132-135, 165.

[24] R. Angus Smith, "Memoir of Dalton," p. 175.

decide on the necessity of fixed composition as a result. But we obtain no results affecting chemical philosophy "[25] In this paper I have shown that Bryan Higgins' theory, far from being "ancient," is a development of Newton's, and that instead of his theory being obscure, and leading to confused ideas regarding chemical composition, it led to a view of the doctrine of fixed proportion, of which the fault was that it was too narrow and rigid.

This difference of opinion, great and hopeless as it may seem, admits of the simplest explanation. Smith's estimate is based upon the "Syllabus" of the year 1775, and upon certain incidental remarks on atoms which he found in the book on "Acetous acid." He was not acquainted with the "Philosophical Essay on Light," which assuredly is not the place where one should expect to find the chemical speculations and ideas regarding atoms, of which nevertheless it is full. Had Angus Smith read this book he must have perceived the clue to the Higgins' ideas, namely, the connection with Isaac Newton. He must then have seen that Bryan Higgins was the first to explain the constant chemical composition of salts in terms of atoms, and that his theory was only too definite and rigid, for it led him to maintain that an acid and an alkali could combine in only one proportion, namely, atom with atom.

W. C. Henry, in his estimate of William Higgins, shows the fatal weakness of failing to see the basis of the theory. Having given Higgins' views regarding the atomic composition of the oxides of nitrogen, he remarks: "It is evident that Mr. Higgins was guided by no fixed and uniform principle, in assigning the atomic constitution of the above compound bodies."[26] This verdict also

[25] *Ibid.*, p. 173.

[26] W. C. Henry, "Memoirs of Dalton," p. 77.

must be set aside. No great penetration of mind is required to divine "the fixed and uniform principle" on which Higgins proceeded in assigning the atomic composition of substances. Although he does not himself mention Newton, there is no room for doubt that Newton's conception of "particles mutually repulsive" was the germ of the theory. Bryan Higgins, who was a student of Newton, made use of this conception, and he communicated it to his nephew. The indebtedness of the nephew to the uncle is as plain as the indebtedness of the uncle to Newton.

There remains now for consideration a remark by Roscoe and Schorlemmer, that "all upholders of an atomic theory" previous to Dalton, "including even [William] Higgins, had supposed that the relative weights of the different elements are the same."[27]

This is a sweeping assertion, of which no proof has ever been offered. One can hardly believe that Newton expressed such an opinion, and it is certain that William Higgins did not. Regarding the oxidation of tin, he supposed that 100 grains of the metal may combine with $7\frac{1}{2}$ or with 15 grains of oxygen.[28] But since he held the oxidation series of an element to be RO, RO_2, RO_3, etc., his figures for tin mean that the atom of the metal was supposed to be much heavier than one atom, or even two of oxygen. Possibly Roscoe and Schorlemmer's statement is based on the case of oxygen and sulphur, which Higgins held to have the same atomic weight. But this conclusion of his depends for one thing on the supposition that the molecule of sulphurous acid (the substance SO_2, not H_2SO_3) is composed of one atom of each element, and for another on

[27] Roscoe and Schorlemmer, "Non-Metallic Elements," p. 35, 1905.

[28] "Comparative View," p. 275.

the experimental fact that the acid is formed by the union of equal weights of the two elements. Higgins proved, quite correctly on his supposition, that the atomic weights of sulphur and oxygen are equal. But proof and assumption are two very different things. Surely it is one thing to prove a result in a particular case, and quite another to assume the result in general.

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V. The Development of the Atomic Theory: (4) Dalton's Physical Atomic Theory.

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In the opinion of the author, many of those who write about Dalton let their attention be engrossed too much by his chemical work. For, in order to understand even the chemical work, it must be kept in mind that Dalton began his scientific career as a meteorologist, that this led him to become a student of physics, and that he took up the study of chemistry subsequently.

The following paper shows that Dalton's physical atomic theory was the first great achievement of his career. It was based on his experimental work, and theory and work together, as soon as published, aroused, in his own words, the "attention of philosophers throughout Europe."

The physical atomic theory, otherwise the theory of "mixed gases," is specially interesting because it marks a stage in the development of Dalton's ideas. Both it and the experiments connected with it arose out of the meteorological observations and studies of his early life. It reveals him as a student of Newton, and as the upholder of a physical atomic theory years before he formed the chemical one.

The present paper is divided into three parts:—I. Dalton's theory of "mixed gases"; II. The beginning and course of Dalton's experimental work; III. The two forms of the physical atomic theory and the dates of their origin.

March 7th, 1911.

I. DALTON'S THEORY OF "MIXED GASES."

The question at issue.

One of the burning questions in science, at the beginning of the nineteenth century, was that of the constitution of "mixed gases." The question could hardly have been discussed much earlier, much less been settled, because the existence of gases, different from atmospheric air and from one another, had not been fully recognised till after the discovery of oxygen in 1774.

The properties of gases are accounted for now by the Kinetic Theory, but this was not established till after the middle of the century. Apart from this theory, men of science explained matters as best they could. The problem naturally arose in connection with the atmosphere, the nitrogen and oxygen of which, although they have different specific gravities, do not separate from one another. Two opinions, says Dalton, arose on this matter: the one supposed the two fluids were "merely mixed together, but assigned no reason why they do not separate . . . . The other supposes a true chemical union to exist between the two, and thus obviates the difficulty arising from the consideration of specific gravity.[1]" The first of these opinions was held by a few isolated individuals. Strange as it must seem now, the chemical explanation of diffusion was not only widespread amongst men of science, but was quite the predominant one.

The germ of Dalton's theory.

Dalton had early shown a tendency in the direction of a mechanical explanation of the state of the atmosphere. The "Meteorological Observations and Essays" published in 1793 contains, as he pointed out many years afterwards,

[1] *Manchester Memoirs*, [1], vol. 5, p. 538, 1802.

"the germs of most of the ideas which I have since expounded more at length in different essays, and which have been considered as discoveries of some importance. For instance, the idea that steam or the vapour of water is an independent elastic fluid . . . . and hence that all elastic fluids, whether alone or mixed, exist independently."[2] He was probably influenced most by Deluc in forming this opinion, but other persons, including Bryan Higgins and Pictet, had expressed views more or less the same as Deluc's.

Dalton's theory of mixed gases.

Thus Dalton had early regarded the constitution of mixed gases from the physical point of view. In the year 1801 he formed a precise theory of his own, which he explained and maintained publicly. The paper in which he describes it, forms one of the set of four experimental essays, which, Dalton himself said, "drew the attention of most of the philosophers of Europe."

He put his theory in the following way: "When two elastic fluids, denoted by A and B, are mixed together, their is no mutual repulsion amongst their particles, that is, the particles of A do not repel those of B, as they do one another."[3] At first the doctrine was not understood, and Dalton had to make further efforts to throw light upon it. His hypothesis meant that while gaseous particles of the same kind repelled one another, there were no forces, whether of repulsion or attraction, between particles of different kinds. Particles of one kind could offer only a passive resistance to the motion of another kind of particles, and acted only as temporary obstacles, in the same way as the pebbles in a stream

[2] "Meteorological Observation and Essays," 2nd Ed., p. v. (1834).

[3] *Manchester Memoirs*, [1], vol. 5. p. 536, 1802.

impede the flow of water. Hence, if two gases were brought together, they were found, sooner or later, to be uniformly mixed.

Dalton and the diffusion of gases.

After the theory had been explained, Dalton deemed it necessary to make new experiments on the diffusion of gases. Priestley, who originally drew attention to this phenomenon, was inclined to think it accidental in its nature. He thought that if "two kinds of air were put into the same vessel with very great care, without the least agitation that might mix or blend them together, they might continue separate."[4] Dalton's experiments, made with the simplest of apparatus, proved, in his own words, "the remarkable fact, *that a lighter elastic fluid cannot rest upon a heavier.*"[5] The importance of this work, by which he established diffusion as a genuine property of gases, was recognised by Berthollet, who carefully repeated it.[6]

Dalton was evidently much gratified by the agreement between his theory and the facts of diffusion. He concludes his memoir on diffusion with a note of triumph :—"The facts, stated above, taken together, appear to me to form as decisive evidence for that theory of elastic fluids which I maintain, and *against* the one commonly received, as any physical principle which has ever been deemed a subject of dispute, can adduce."[7]

Dalton's theory and the vapour of water.

Obviously, a special case of the mixed gases question is that of the water vapour in the atmosphere. The

[4] "Experiments and Observations, &c.," abridged, vol. 2, p. 441.

[5] *Manchester Memoirs*, [2], vol. 1, p. 260, 1805.

[6] *Mém. d'Arceuil*, vol. 2, p. 463, 1809.

[7] *Manchester Memoirs*, [2], vol. 1, p. 270, 1805.

general, though not the universal opinion was, that this vapour was present in a state of combination with the air. The evaporation of water was thought to be an act of chemical combination between air and water, whilst boiling was a physical action. For since the atmospheric pressure prevents water from boiling at ordinary temperatures, it was thought that boiling was something quite distinct from evaporation, which takes place at all temperatures and pressures of the air. This distinction had received the sanction even of Lavoisier.[8]

Dalton's theory had a special bearing on this subject. For the theory meant that the pressure of a mixture of gases is the sum of the respective pressures of the gases in the mixture. Dalton saw that the water vapour in the atmosphere had to be considered in terms of the pressure of the vapour. Experimentally he showed that the evaporation of water is proportional to the pressure of the vapour which the water gives off. At any given temperature there is a maximum which this pressure can reach, and water, whether in contact with the air or not, can evaporate till the pressure of its vapour reaches this maximum and no further. On the other hand, air in which the water vapour is not at this maximum pressure can be cooled till the maximum is reached, and then, on further cooling, the water is deposited as dew. This led to observations of the "dew-point," which Dalton was the first to institute.

It was thus in the direction of Meteorology that Dalton's theory first bore fruit. In this science, as Playfair has pointed out, it is easier than in any other to "accumulate observations, and more difficult to ascertain principles." At the beginning of the nineteenth century, by pointing out the significance of the dew-point, Dalton

[8] "Traité Elémentaire de Chimie," 3rd ed., pp. 7-11, 39.

succeeded in transforming hygrometry, and "raising it to the rank of an exact science."[9]

Dalton's theory and Henry's law.

Dalton's theory had been only a short time before the world, when it was reinforced in a remarkable way. It was found to have an important bearing on the solubility in water of a gas under various pressures. The study of this subject had been undertaken by William Henry, already mentioned in the second paper of this series as a friend of Dalton.

Henry had discovered the law, which is now called after him, that at a given temperature, "water takes up the same volume of condensed gas as of gas under ordinary pressure."[10] The amount dissolved is proportional to the pressure. This, as Dalton pointed out to Henry, is a strong argument in favour of the view that solution is "purely a mechanical effect." If gas, in a state of absorption by water, is retained entirely by the incumbent pressure, there is no need to call in the notion of chemical affinity.

Not only so, but in the matter of the solubility of a mixture of gases, Dalton's theory proved able to sustain a severe enough test. Henry found that each gas dissolved in water as if the others were absent. "Each gas," he concluded, "when dissolved in water, is retained in its place by an atmosphere of no other gas but its own kind."[11] This is precisely what was to be expected from Dalton's theory.

Henry had opposed the theory when it was first made known. He now wrote Dalton a letter, which was read

[9] See W. C. Henry, "Memoirs of Dalton," p. 226.

[10] *Phil. Trans.*, p. 41, 1803.

[11] *Nicholson's Journ.*, [2], vol. 9, p. 126, 1804.

before this Society and then published, expressing his entire satisfaction with it. "In the discussions...which took place in the Society on your several papers, the doctrine of mixed gases was opposed by almost every member interested in such subjects, and by no one more strenuously than myself. I am now satisfied that.... your theory is better adapted than any former one, for explaining the relation of mixed gases to each other, and especially the connection between gases and water."[12]

This support must have been specially gratifying to Dalton, in view of the keen opposition and criticism which the theory was receiving in other quarters. It probably confirmed and enhanced the "almost life-long friendship" between the two men, which is referred to repeatedly in this series of papers.

The "mixed gases" controversy.

The controversy which was aroused by Dalton's theory of mixed gases affords proof at once of the interest taken in his mechanical explanation of the phenomenon, and of the tenacity with which the chemical explanation was adhered to. The view that air is a chemical compound was maintained with a persistency which is hardly credible now, and which throws into relief the originality and vigour of mind which Dalton showed in forming and urging a wiser view. The balance of opinion was against him, for his opponents included Claude Louis Berthollet, Thomas Thomson, John Gough, John Murray, and Humphry Davy.

Dalton's contention, that the diffusion of gases is a physical phenomenon, was at length fully and finally recognised in the Kinetic Theory of Gases. Meantime Dalton had to do his best in the circumstances, and the

[12] *Nicholson's Journ.*, [2], vol. 8, p. 297, 1804.

particular mechanism by which he accounted for diffusion proved specially vulnerable.[13]

The most eager critic of the mechanical explanation was Gough. He wrote numerous letters and essays against it, which were answered by Dalton, and on one occasion by Henry. One of his criticisms was acute. If, as Dalton supposed, the particles of oxygen in the atmosphere have no action on the particles of nitrogen, and *vice versa*, this must affect the transmission of sound. Gough said the oxygen must transmit one sound wave and the nitrogen another, each with its own velocity, so that at a sufficient distance a sound should be heard double.

Berthollet, in his "Essai de Chimie Statique," shows himself a whole-hearted believer in the chemical theory. "It appears to me incontrovertible, that it is a true chemical action which produces the solution of liquids in gases, and evaporation."[14] He was unfavourably impressed by the diagram appended to the "Mixed Gases" Essay, in which Dalton exhibits particles of oxygen, nitrogen, water and carbon dioxide existing in the atmosphere independently of one another.[15] "A diagram in which Dalton has attempted to show how different gaseous molecules may be disposed in the same space, is... only a figment of the imagination."[16]

Thomas Thomson's interest was roused to a high pitch by Dalton's theory. Whilst expressly withholding his assent to it, he noticed it in edition after edition of

[13] As a matter of fact, Dalton did for years believe that "portions of gas of different kinds behave to each other in a different manner from portions of gas of the same kind . . . whereas there is no difference between the two cases." Clerk Maxwell, "Theory of Heat," 10th ed., pp. 28-29.

[14] *Op. cit.*, § 164.

[15] *Manchester Memoirs*, [1], vol. 5, p. 602, 1802.

[16] *Op. cit.*, § 244.

his "System of Chemistry." Dalton's reply to the criticism in the second edition had a notable consequence. Thomson visited Manchester in order to get an explanation of the theory from the author himself, and it was on this occasion that Dalton told him about the chemical atomic theory.

II. The beginning and course of Dalton's experimental work.

The Beginning.

Dalton did not begin original experimental work till 1799, when he was thirty-three years of age, and had been six years in Manchester. Up to then he had confined himself to work of observation, chiefly in meteorology. The first paper in which his own experiments occupy a considerable space is his memoir on the power of fluids to conduct heat. It was read before this Society on the 12th April, 1799.

A previous paper of his, read six weeks earlier, is of quite another stamp. The title of this is as follows:— "A paper, containing Experiments and Observations to determine whether the quantity of Rain and Dew is equal to the quantity of water carried off by the rivers and raised by Evaporation; with an inquiry into the origin of springs." Now, not only are the experiments recorded in this paper hardly worthy of the name, but the subject itself is of the nature of a forlorn hope. Dalton could not have embarked on such a hopeless inquiry as this, if he had been accustomed to experimental research, and had experienced the advantages to be gained simply by limiting the scope of an investigation. This paper, therefore, marks the end of the first stage in his scientific career. By April of the year 1799 he was in the full swing of experimental work.

Experiments connected with the vapour pressure of water.

A paper which Dalton read April 18th, 1800, marks another stage on the way. The title, which is significant of much, runs as follows:—"Experimental Essays, to determine the Expansion of Gases by Heat, and the *maximum* of steam or aqueous vapour, which any gas of a given temperature can admit of; with observations on the common or improved Steam Engines."

On this title four remarks may be made. (1) Dalton had arrived by April, 1800, at the idea, which forms the central fundamental conception of the second and third of the "Experimental Essays" of October, 1801, of the vapour pressure of water. He had begun to consider other gases besides the air, and knew that the maximum of water vapour in any gas is independent of the nature of the gas. It was in order to show this at different temperatures that he began to measure the "expansion of gases by heat."

(2) There is a practical connection between the expansion of gases by heat, and the original topic of the water vapour in the atmosphere. Dalton's explanation of the discrepancies between the results of earlier workers on the subject is that it "arose from the want of due care to keep the apparatus and materials free from moisture."[17]

(3) This paper, although passed for publication by the Society, never appeared. Nothing remains of the "Observations on the common or improved Steam Engines."

(4) Perhaps Dalton had discovered by April, 1800, what we know as Charles' law, that different gases have the same expansion by heat. But he does not make this claim himself. The law forms the subject of the fourth of the "Experimental Essays," and this fourth essay,

[17] *Manchester Memoirs*, [1], vol. 5, p. 596, 1802.

though usually dated October, 1801, was, as a matter of fact, not read then as the first three were before this Society.

Later developments.

Between the paper of April, 1800, and the "Experimental Essays" of October, 1801, Dalton took up the study of only two additional topics. One of these, the vapour pressure of other liquids than water, was a natural outcome of previous work, and calls for no special comment here. The other was that of the explanation of the phenomena of mixed gases. This, a large topic and not an experimental one, is discussed in the last section of this paper. But here is the place to point out that Dalton's reflections on this subject led to two experimental inquiries of the greatest consequence. One of these, already mentioned in this paper, was the study of the diffusion of gases. The other was the determination by Dalton of the composition of the atmosphere, the outcome of which, as will be shown in the next paper, was the formation of the chemical theory.

III. The two forms of the physical theory and the dates of their origin.

The date of the first diffusion hypothesis.

Dalton, in the Introduction to his set of four "Experimental Essays" of October, 1801, explains that this theory of mixed gases was arrived at after his other results. "The first law [*i.e.*, the mixed gases theory] which is as a mirror in which all the experiments are best viewed, was *last* detected, and after all the particular facts had been previously ascertained."[18]

[18] *Manchester Memoirs*, [1], vol. 5, p. 536.

There is no reason to question this statement. It is true that Dalton's historical narratives, as has been shown in the second paper of this series, cannot be accepted at their face value. But this is a contemporary statement, and, as such, must receive a considerable degree of credit.

The physical theory was formed between April, 1800, and September of the following year. There is no hint of it in the title of the paper which Dalton read on the 18th April of the earlier year. Again, the date of the first sketch of the theory, which he sent to *Nicholson's Journal*, is the 14th September, 1801, and the theory can hardly have arisen earlier than August. It is true that Angus Smith assigns the reading of the essay "On the Constitution of Mixed Gases" to July 31st, and October 2nd and 16th for the reading of the 2nd and 3rd essays respectively.[19] But the dates mentioned at the head of the papers in the *Manchester Memoirs*, are the 2nd, 16th, and 30th October. Dalton must have known the dates on which his own papers were read, and as the author he was interested in not dating them later than was necessary. In the Minute-book of the Society the title of each of these papers was entered on a left hand page, and the date and other particulars of the meeting at which the paper was read on the right hand page. Angus Smith has made the slip, which one can easily understand, of assigning the reading of a paper to the meeting minuted on the previous page.

The influence of Newton on Dalton.

The theory was formed under a new influence. Between April, 1800, and August or September, 1801

[19] Angus Smith, "Memoir of Dalton," p. 254. These are not the only wrong dates in his list of Dalton's papers.

Dalton came under the stimulus of Newton's atomic theory. Everything goes to show that this had a great effect on him. He hardly mentions Newton in his early writings. In 1801, and subsequently, he quoted Newton on every suitable occasion, and in particular he mentions the 23rd Proposition of the 2nd Book of the "Principia" at least five times. The mutually repulsive particles of this proposition play their part in Dalton's theory. The wording of it shows this:—"When two elastic fluids, denoted by A and B are mixed together, there is no mutual repulsion amongst their particles; that is the particles of A do not repel those of B, as they do one another."[20]

Dalton's theory is a true development of the theory of Newton, in respect that it is a static one, representing the atoms as being, ultimately, at rest among themselves. If, as was shown in the 3rd paper of this series, Newton in forming his theory deliberately set aside the dynamic ideas of Descartes, it is to be remembered that these ideas at length found expression in the Kinetic Theory of Gases.

The amended diffusion hypothesis.

As already stated in the second paper of this series, Dalton explained in a lecture which he gave in 1810, that he had not at first contemplated the effect of *difference of size* in the particles of elastic fluids. But he reflected that if the sizes be different, then on the supposition that the repulsive power is heat, no equilibrium can be established by particles of different sizes pressing against each other." On consideration, he found "that the *sizes must* be different;" "thus," he concludes, "we arrive at the reason for that diffusion of every gas through every other gas,

[20] *Manchester Memoirs*, [1], vol. 5, p. 536, 1802.

without calling in any other repulsive power than the well known one of *heat*." This, he says, occurred in 1805.[21]

In later life, Dalton gave up this amended hypothesis, and reverted to his original one.[22] But in 1808 he expounded them both in the "New System." This may seem inconsistent of him, inasmuch as the two hypotheses are different from one another. Yet they are both forms of the physical atomic theory. Dalton's consistency lies in his adherence to a mechanical hypothesis in contrast to a chemical one. The question of the precise mechanism was subsidiary, and the mixed gases controversy turned entirely on the theory which Dalton advanced in 1801. No one took any notice of his change of front. It has, therefore, not been necessary to consider the amended diffusion hypothesis till now. The hypothesis is less important for its own sake than in its bearing, or supposed bearing, on the development of Dalton's chemical theory.

The amended hypothesis and the chemical atomic theory.

In the lecture already quoted, Dalton connects this amended hypothesis with the genesis of his chemical atomic theory. "The different *sizes* of the particles of elastic fluids under like circumstances . . . . being once established, it became an object to determine the relative *sizes* and *weights* together with the relative *number* of atoms in a given volume. This led the way to the combination of gases . . . . Thus a train of investigations was laid for determining the *number* and *weight* of all chemical elementary principles which enter into any sort of combination with one another."[23]

[21] Roscoe and Harden, "New view of the origin of Dalton's Atomic Theory," pp. 16-17.

[22] *Phil. Trans.*, 1826, part 2, p. 174.

[23] Roscoe and Harden, *loc. cit.*

"This led the way to the combination of gases." Undoubtedly the combination of gases was the basis of Dalton's chemical theory, and the gist of his narrative is, that he *first* concluded the particles of different gases to be different in size, and *subsequently* arrived at his chemical theory.

1805 or 1803?

Roscoe and Harden, instead of taking this narrative as a document requiring interpretation in the light of the available information, and above all, in the light of Dalton's habit of mind, have accepted it at its face value. Even then they are compelled to admit there is something wrong. The note-books shew that the chemical theory was formed in 1803, and if the amended diffusion hypothesis was formed previously, then the date, 1805, which Dalton gives, must be wrong. Roscoe and Harden conclude that the date of the amended hypothesis is 1803.[24]

The date is 1804.

There are two grave objections to the supposition that the theories were formed in the order given by Dalton. One of these is based on the nature of the theories, and will be considered in the next paper. The other has to do with the genesis of the diffusion hypothesis. Roscoe and Harden have failed to quote from the note-book the passage which deals with this. It is as follows:—

"*On the ultimate atoms of elastic fluids.*

"There are but three positions that are any way likely to be true on this head.

"1. The ult. atoms of all gases are of the same weight.

[24] *Op. cit.*, p. 25.

"2. The ult. atoms are of the same relative weight as the gases themselves.

"3. That neither of these positions is accurate.

"According to the first the gases of greatest specific gravity are those whose particles are closest and the diameters of the elastic particles will be as the cube root of the sp. gr. This cannot be true for nit. gas which is made up of azot and oxygen is lighter than oxygen itself; and so is aq. vapour than oxygen one of its constituents."[25]

"According to the 2nd position all gases will have the same number of particles, and consequently the same distances of each in a given volume, under like circumstances. This position is contradicted by facts: for all compounds would be heavier than their simples upon this principle, which is contrary to experience.

"The two former positions being disproved, it follows that when two gases of like force, &c., are presented to each other, the number of particles in a given surface of one of them will not be the same as in the other; consequently, no proper equilibrium can take place."[26]

This material is as important as anything on the subject can well be. The pages quoted, Nos. 109 and 111 of the note-book, amount to a summary of Dalton's reasoning on the subject of the sizes of atoms, leading to his decision in favour of the new diffusion hypothesis. It is easy to assign a date to this decision. By reason of the subject-matter, pages 107, 109, and 111 are closely connected with one another. Page 107 contains a table of the weights and diameters of atoms, a table which, it may well be supposed, was drawn up in order to illustrate Dalton's inquiry into the sizes of atoms. It is dated

[25] Note-books, vol. 1, p. 109.

[26] *Op. cit.*, p. 111.

September 14th, 1804, and this is the approximate date of the amended diffusion theory.

Dalton probably influenced by Thomson and Gough.

Up to this time Dalton had given no sign of anything but the fullest confidence in his original theory. He had defended it eagerly, and, as late as June, had been encouraged in his belief by the accession of William Henry to his side. What then could have induced Dalton, not a very impressionable man, to reconsider the matter?

It may be assumed, in the absence of any positive information on the subject, that the change was due partly to Thomas Thomson and partly to John Gough. As has already been mentioned more than once in these papers, Thomson visited Manchester with the express object of discussing the mixed gases theory with Dalton. Now everything goes to show that Thomson made a considerable impression on him and won his confidence. He explained the chemical theory to Thomson in detail, and afterwards mentioned Thomson's opinions regarding mixed gases, although adverse to his own, with the utmost respect.[27] Consequently one can well believe that Thomson's scepticism regarding the original mixed gases theory began to shake his confidence in it. Again, John Gough had written two letters, which appeared in *Nicholson's Journal*, criticising the theory. The criticism was effective, for Dalton, although he continued to maintain his theory, made no answer at the time to Gough's argument regarding the velocity of sound. Gough's letters are dated July 16th and August 23rd, 1804, respectively. The interview between Dalton and Thomson occurred on

[27] "New System of Chemical Philosophy," 1808, p. 72.

the 27th August, and Dalton's reply to Gough is dated 8th September. Thus Thomson's objections to the theory, with Gough's in addition, may have compelled Dalton to reconsider the matter. There was time for reconsideration between the 8th September and the 14th, the date of Dalton's decision to put the explanation of the diffusion of gases on a new basis.

The principal references connected with the theory of mixed gases.

1792.

1. "On Evaporation," by JEAN ANDRÉ DELUC. *Phil. Trans.*, p. 400.

1793.

2. "Meteorological Observations and Essays," by JOHN DALTON.

1799.

3. "Experiments and Observations, to determine whether the quantity of Rain and Dew is equal to the quantity of Water carried off by the rivers and raised by evaporation; with an Inquiry into the Origin of Springs," by JOHN DALTON. Read* March 1st. Pub. *Manchester Memoirs*, vol. 5, part 2, p. 346, 1802. (The footnote, p. 351, was added after the paper was read.)

1800.

4. "Experimental Essays, to determine the Expansion of Gases by Heat, and the maximum of Steam or Aqueous Vapour, which any Gas of a given temperature can admit of; with observations on the common and improved Steam Engines," by JOHN DALTON. Read April 18th. Never published in full; see no. 8.

* In the above list, "read" means read before the Manchester Literary and Philosophical Society.

1801.

5. "New theory of the constitution of mixed aeriform fluids, and particularly of the atmosphere," by JOHN DALTON. Written September 14th. Pub. *Nicholson's Jour.*, [1], vol. 5, p. 241.

6-9. "Experimental Essays on the Constitution of mixed gases; on the force of steam or vapour from water and other liquids in different temperatures, both in a torricellian vacuum and in air; on evaporation; and on the expansion of gases by heat," by JOHN DALTON. The 1st of these four essays was read Oct. 2nd, the 2nd Oct. 16th, the 3rd Oct. 30th. Pub. *Manchester Memoirs*, [1], vol. 5, p. 535, 1802.

1802.

10. "System of Chemistry," by THOMAS THOMSON, 1st ed., vol. 3, p. 270.

11. "New theory of the constitution of mixed gases elucidated," by JOHN DALTON. Written Nov. 18th. Pub. *Nicholson's Jour.*, [2], vol. 3, p. 267. (Chiefly called forth by no. 10.)

12. "Experimental Inquiry into the Proportion of the several gases or elastic fluids constituting the atmosphere," by JOHN DALTON. Read Nov. 12th. Pub. *Manchester Memoirs*, [2], vol. 1, p. 244, 1805.

1803.

13. "Essai de Chimie Statique," by C. L. BERTHOLLET (especially §§ 158, 160, 163, 164, 171, 240, 242—244).

14. "On the tendency of elastic fluids to diffusion through each other," by JOHN DALTON. Read Jan. 28th. Pub. *Manchester Memoirs*, [2], vol. 1, p. 259, 1805.

15. "On the absorption of gases by water and other liquids," by JOHN DALTON. Read Oct. 21st. Pub. *Manchester Memoirs*, [2], vol. 1, p. 271, 1805.

16. "An Essay on the theory of mixed gases and the state of water in the atmosphere," by JOHN GOUGH. Read Nov. 4th. Pub. *Manchester Memoirs*, [2], vol. 1, p. 296, 1805.

17. "A reply to Mr. Dalton's objections to a late theory of mixed gases," by JOHN GOUGH. Written Dec. 2nd. Read Jan. 27th, 1804. Pub. *Manchester Memoirs*, [2], vol. 1, p. 405, 1805.

18. "Appendix to Mr. WILLIAM HENRY'S paper, on the quantity of gases absorbed by water, at different temperatures, and under different pressures." *Phil. Trans.*, p. 274.

1804.

19. "System of Chemistry," by THOMAS THOMSON, 2nd ed., vol. 3, p. 316.

20. "On the supposed chemical affinity of the elements of common air; with remarks on Dr. Thomson's observations on that subject," by JOHN DALTON. Written June 16th. Pub. *Nicholson's Jour.*, [2], vol. 8, p. 145. (A reply chiefly to no. 19.)

21. "Illustration of Mr. Dalton's theory of the constitution of mixed gases," in a letter from Mr. Wm. Henry, of Manchester, to Mr. Dalton. Written June 20th, read June 29th. Pub. *Nicholson's Jour.*, [2], vol. 8, p. 297.

22. "On the solution of water in the atmosphere, and on the nature of atmospherical air," by JOHN GOUGH. Written July 16th. Pub. *Nicholson's Jour.*, [2], vol. 8, p. 243.

23. "Strictures on Mr. Dalton's doctrine of mixed gases, and an answer to Mr. Henry's defence of the same," by JOHN GOUGH. Written Aug. 23rd. Pub. *Nicholson's Jour.*, [2], vol. 9, p. 52. (A reply to nos. 20 and 21.)

24. "Observations on Mr. Gough's strictures on the doctrines of mixed gases, &c.," by JOHN DALTON. Written Sept. 8th. Pub. *Nicholson's Jour.*, [2], vol. 9, p. 89. (A reply to nos. 22 and 23.)

25. "Atmospherical air not a mechanical mixture of the oxigenous and azotic gases, demonstrated from the specific gravities of these fluids," by JOHN GOUGH. Written Sept. 5th. Pub. *Nicholson's Jour.*, [2], vol. 9, p. 107.

26. Letter to the Editor from Mr. William Henry in reply to Mr. Gough. Written Sept. 13th. Pub. *Nicholson's Jour.*, [2], vol. 9, p. 126. (In reply to nos. 22 and 23.)

27. "Reply to Mr. Dalton on the constitution of mixed gases," by JOHN GOUGH. Written Oct. 16th. Pub. *Nicholson's Jour.*, [2], vol. 9, p. 160. (A reply to no. 24.)

28. "Observations on Mr. Gough's two letters on mixed gases," by JOHN DALTON. Written Nov. 15th. Pub. *Nicholson's Jour.*, [2], vol. 9, p. 269 (in reply to nos. 25 and 27).

29. "Further observations on the constitution of mixed gases," by JOHN GOUGH. Written Dec. 13th. Pub. *Nicholson's Jour.*, [2], vol. 10, p. 20, 1805.

1805.

30. "Remarks on Mr. Gough's two Essays on Mixed Gases, and on Professor Schmidt's experiments on the expansion of dry and moist air by heat," by JOHN DALTON. Read Oct. 4th. Pub. *Manchester Memoirs*, [2], vol. 1, p. 425 (a reply to nos. 16 and 17).

1806.

31. "System of Chemistry," by JOHN MURRAY. Vol. 2, pp. 48-53, and note E.

1807.

32. "System of Chemistry," by THOMAS THOMSON. 3rd ed., vol. 3, p. 440.

33. "On the Chemical composition of the Atmosphere." Works, vol. 8, pp. 252-255, by HUMPHRY DAVY.

1808.

34. "New System of Chemical Philosophy," by JOHN DALTON, pp. 150-208.

1809.

35. "Experiments on the expansion of moist air raised to a boiling temperature," by JOHN GOUGH. Written May 22nd. Pub. *Nicholson's Jour.*, [2], vol. 23, p 182.

36. "Sur le mélange réciproque des gaz," par C. L. BERTHOLLET. *Mém. d'Arceuil*, vol. 2, p. 463 (a repetition of Dalton's experiments in no. 14).

1826.

37. "On the constitution of the Atmosphere," by JOHN DALTON. *Phil. Trans.*, part 2, p. 174.

1837.

38. "Sequel to an Essay on the constitution of the Atmosphere," by JOHN DALTON. *Phil. Trans.*, p. 347.

1834.

39. Dr. Prout's reply to Dr. W. Charles Henry. Written July 18th. *Phil. Mag.*, [3], vol 5, p. 133.

1844.

40. "Observations on the Diffusion of Gases," by T. S. THOMSON. *Phil. Mag.*, [3], vol. 25, p. 51.

1842.

41. "Elements of Chemistry," by THOMAS GRAHAM, pp. 69, 70, 71, 75.

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VI. The Development of the Atomic Theory: (5) Dalton's Chemical Theory.

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Introduction.

In the year 1801 Dalton's physical atomic theory (described in the fourth paper of this series) was devised as an explanation of the diffusion of gases. Since the prevailing tendency of the time had been to regard diffusion as due to chemical affinity between the gases concerned, Dalton was forced to consider carefully the nature of physical and chemical changes, and to draw a distinction between them. His own theory of diffusion turned on this distinction. Thus, in the course of his argument against the supposition that diffusion is due to chemical affinity, he asks the question, "Why do not oxygenous and azotic gases, taken in due proportion and mixed, constitute nitric acid gas, another elastic fluid, totally distinct in its properties, from either of the ingredients."[1] Obviously, therefore, whilst Dalton's attention was being directed *principally* to physical phenomena, he had in his mind a distinct conception of chemical change.

The object of this paper is to consider how Dalton passed from the physical atomic theory, which was formed first, to the chemical one, which was formed afterwards. The author has already shown, in the second paper of this series, that the various narratives we possess of the origin of the chemical theory, can be traced back to

[1] *Manchester Memoirs*, vol. 5, pp. 538-539, 1802.

Dalton himself. This is simply what was to be expected in the nature of the case. Moreover, since Dalton was inconsistent in the matter, no single account of his can be accepted at its face value. The version of the origin which is advanced in this paper, consequently, need not be rejected off-hand, as not having received the sanction of Dalton. It is offered as a fair account of the present state of our knowledge, on a matter on which absolute certainty is not yet attainable.

The paper is divided into two parts :—I. The principles of Dalton's theory ; II. The genesis of the theory.

I. The principles of Dalton's theory.

The first table of atomic weights.

For the present purpose of studying the origin of the chemical theory, Dalton's note-books contain material of inestimable value: they afford facts which cannot be disputed. Under date 6th September, 1803, there is an atomic weight table of the highest interest. It is quoted by Roscoe and Harden as follows :—*

Ult. at. hydrogen	1
„ „ oxygen	5·66
„ „ azote	4
„ „ carbon	4·5
„ „ water	6·66
„ „ ammonia	5
„ „ nitrous gas... ...	9·66
„ „ „ oxide ...	13·66
„ „ nitric acid	15·32
„ „ sulphur	17
„ „ sulphurous acid ...	22·66
„ „ sulphuric „ ...	28·32
„ „ carbonic „ ...	15·8
„ „ oxide of carbon ...	10·2

* "New View of the Origin of Dalton's Atomic Theory," p. 28.

This table of atomic weights is of extraordinary interest because, besides being the earliest known, it is based on the same ideas as the one published in the "New System" five years later. There is only one change: sulphurous and sulphuric acids in the earlier table are virtually SO and SO_2 respectively, and in the later table they are SO_2 and SO_3. But this does not affect the fact that after the table was drawn up in 1803, Dalton made no essential change in the theory. The principles of 1803 remain as nearly as possible unchanged in 1808, so far as one can judge of principles by results. In the one scheme just as in the other, the compound atom of water consists of 1 atom of hydrogen and 1 of oxygen, that of ammonia of 1 of hydrogen and 1 of nitrogen. Nitrous gas is virtually NO, nitric acid is NO_2, nitrous oxide N_2O, carbonic oxide is CO, carbonic acid is CO_2.

Debus on the "Dalton-Avogadro" hypothesis.

Debus has devoted a series of papers to the study of the principles on which Dalton arrived at chemical formulæ and atomic weights. The whole series may be said to depend on the assumption that Dalton deliberately made a mystery of the evolution of his theory. "Der geniale Baumeister hat sorgfältig alle Werkzeuge und Pläne entfernt und zeigt ohne einleitende Bemerkungen sofort das fertige Gebäude."[2] This is the kind of statement which ought not to be made except as the result of an exhaustive study of the available material. Everyone must admit that the subject is obscure, but, as will appear in the course of this paper, there is little justification for saying that Dalton deliberately (sorgfältig) made it so. The true explanation of the obscurity is that the task of

[2] *Zeitsch. physik. Chem.*, 1899, vol. 29, p. 266.

considering how his own ideas had arisen was uncongenial to him, and he never devoted his mind to it.

As the result of his studies, Debus concluded that Dalton was greatly influenced, during the development of his atomic theory, by the supposition that the particles of different gases under similar conditions are of the same size. This doctrine, which is usually known as Avogadro's hypothesis, Debus calls the "Dalton-Avogadro" hypothesis.

Debus first advanced this belief of his in a pamphlet entitled, "Ueber einige Fundamentalsätze der Chemie, insbesondere das Dalton-Avogadrosche Gesetz" (1894, Cassel). His opinion having been controverted by Roscoe and Harden, in their "New View of the Origin of Dalton's Atomic Theory," he replied, and a controversy ensued, in which G. W. A. Kahlbaum also took part. The series of papers is as follows:—Debus, *Zeitsch. physikal. Chem.*, vol. 20, p. 359, 1896 (or *Phil. Mag.*, vol. 42, p. 350, 1896); Roscoe and Harden, *Zeitsch. physikal. Chem.*, vol. 22, p. 241, 1897 (or *Phil. Mag.*, vol. 43, p. 153, 1897); Debus, *Zeitsch. physikal. Chem.*, vol. 24, p. 325, 1897; vol. 29, p. 266, 1899; Kahlbaum, *Zeitsch. physikal. Chem.*, vol. 29, p. 700, 1899; Debus, *Zeitsch. physikal. Chem.*, vol. 30, p. 556, 1899.

Debus can justify his belief in two ways:—(1) Dalton certainly stated in 1808 that he once had a sort of belief in the hypothesis in question. "At the time I formed the theory of mixed gases, I had a confused idea, as many have, I suppose at this time, that the particles of elastic fluids are all of the same size; that a given volume of oxygenous gas contains just as many particles as the same volume of hydrogenous; or if not, that we have no data from which the question could be solved. But . . . .

I became convinced that different gases have *not* their particles of the same size."[3]

(2) Debus argues, from a phrase in Thomas Thomson's first sketch of the atomic theory, that Dalton was still in 1804 a believer in the hypothesis. This is the phrase, "the density of the atoms."

The "density of the atoms."

The interpretation of this phrase is open to question, and Roscoe and Harden do not agree with Debus on the matter. But neither they nor any of the parties to the controversy seem to be aware that Dalton put exactly the same construction on the phrase as Debus, and at the same time repudiated the opinion which it attributed to him. "It is rather amusing to me to observe the different manners in which a cursory view of the atomic system strikes different observers. Dr. Thomson . . , used the phrase *density of the atoms* indifferently for *weight of the atoms*, thereby implying that all atoms are of the *same size*, and differ only in *density;* but he has since very properly discontinued the use of the phrase."[4]

It is, of course, impossible that a statement, made by Dalton in 1814, can be taken to prove that he did not use a misleading expression in a conversation held ten years earlier. He may have used the phrase in question in his interview with Thomson, or Thomson may have originated the phrase. These are the two possibilities. But the matter is not one of high importance. There are far stronger arguments than this statement of the year 1814 can be, against the opinion held by Debus.

[3] "New System of Chemical Philosophy," 1808, pp. 187-188.

[4] *Ann. of Phil.*, vol. 3, p. 175, 1814.

Dalton practically ignores the hypothesis.

The really important question is, the sense in which Dalton held this hypothesis. Did he perceive and consider all its consequences, immediate and remote, and did he, in any way, act upon his belief in it? That he did none of these things is the plain meaning of the passage in which he speaks of his holding the hypothesis as a "confused idea."

There are four different ways in which Dalton might have applied the hypothesis, or drawn deductions from it:

1. The hypothesis is to the effect that particles of nitrogen and oxygen are of the same size. Dalton's first explanation of diffusion was that particles of oxygen neither attract nor repel those of nitrogen. Between these two opinions there is no necessary connection. He did not hold the diffusion theory as a logical consequence of the hypothesis, and he did not even specify the hypothesis in his explanation of the theory.

2. Dalton did not use the hypothesis as a means of arriving at atomic weights and formulæ. He used for that purpose the 1 : 1 rule, which led him to the formula OH for water, whilst the hypothesis must have led to the formula H_2O. Thomson, in his first sketch of the theory, says expressly that the 1 : 1 rule was "the hypothesis on which the whole of Mr. Dalton's notions . . . is founded."[5]

3. What is known as Gay-Lussac's law, regarding the combining volumes of gases, is a necessary consequence of the hypothesis. This everyone must admit. Yet Dalton did not at once deduce the law from the hypothesis, and when at length he did so, and endeavoured to test it experimentally, he regarded his results as dis-

[5] "System of Chemistry," 3rd edition, 1807, vol. 3, p. 424.

proving both doctrines. Debus, as the author has pointed out elsewhere, has committed himself to the opinion that Dalton could be at one and the same time a believer in the hypothesis and not in the law.[6]

4. Finally, Dalton held this hypothesis without considering that it leads to the conclusion, familiar now to chemists, that the "atoms" of hydrogen, oxygen and other elements are divisible.

There is no evidence, not the faintest indication, that Dalton had realised the hypothesis before the end of the year 1803, in any one of these four ways. It is, therefore, impossible to suppose that the hypothesis—the "confused idea"—had any influence on him whilst he was forming his chemical atomic theory.

The main principles of Dalton's system.

The principles on which Dalton based his theory must have continued the same from 1803 to 1808, simply because his opinions regarding the "atom" of water, of ammonia, etc., remained the same. The general principles regarding the combination of atoms, which he set out in 1808, are somewhat cumbrous, and some of them superfluous. They can be reduced to two:—(1) That atoms of different kinds tend to combine in the proportion 1 : 1 rather than in any other, that the next proportion to occur is 1 : 2, then 1 : 3, and so on ; (2) that when two compounds of the same two elements are gaseous, the lighter is binary and the heavier tertiary.

It is true that this second principle is not to be found among the set of rules which Dalton gives in the "New System of Chemical Philosophy." He says there that

[6] "Avogadro and Dalton—the standing in Chemistry of their hypotheses," 1904, pp. 63-66.

"a binary compound should always be specifically heavier than the mere mixture of its two ingredients" [compounds and ingredients being supposed to be gaseous]. This rule is open to two objections:—(1) It is not true, as the case of hydrochloric acid shows; (2) it is of no use and was not used for the problem that Dalton had to solve. It cannot be used to ascertain whether the two gaseous oxides of carbon ought to receive the formulæ CO and CO_2 respectively, or C_2O and CO. In the 2nd part of the "New System" he says:—"carbonic acid is of greater specific gravity than carbonic oxide, and on that account it may be presumed to be the ternary or more complex element [*sic*]. It must, however, be allowed that this circumstance is rather an indication than a proof of the fact."[7] One can well believe that it was on this principle Dalton arrived at the molecular constitution of these gases, and of nitric and nitrous oxides as well, in the year 1803.

The connection between the physical and the chemical theories.

The first rule has been called the rule of "greatest simplicity," not only in allusion to its character, but as meaning that it is based on the instinct for simplicity and needs no other justification. As a matter of fact Dalton deduced it from first principles. Dr. Bostock, in the course of a criticism of the atomic theory, raised the question, "When bodies unite only in one proportion, whence do we learn that the combination must be binary?"

In answer Dalton gave an explanation, which shows that Newton's postulate of similar particles, which are "mutually repulsive," was the fundamental idea of the

[7] "New System of Chemical Philosophy," 1810, p. 369.

chemical as it had been of the physical atomic theory. "When an element A has an affinity for another B, I see no mechanical reason why it should not take as many atoms of B as are presented to it, and can possibly come into contact with it, . . . except in so far as the repulsion of the atoms of B among themselves are [*sic*] more than a match for the attraction of an atom of A. Now this repulsion begins with 2 atoms of B to 1 of A, in which case the 2 atoms of B are diametrically opposed; it increases with 3 atoms of B to 1 of A, in which case the atoms of B are only 120° asunder . . . . and so on in proportion to the number of atoms. It is evident then from these positions, that, as far as powers of attraction and repulsion are concerned (and we know of no other in chemistry) . . . *binary* compounds must first be formed in the ordinary course of things, then *ternary* and so on, till the repulsion of the atoms of B . . . refuse to admit any more."[8]

Consequently, Newton's postulate of similar particles which are mutually repulsive, is the basis of both the physical and the chemical atomic theories of Dalton.

II. The Genesis of the Chemical Theory.

The inductive and deductive accounts of the genesis.

This discussion of principles, however, does not exhaust the subject. Much remains obscure regarding the train of thought which Dalton followed in passing from the physical to the chemical theory. The crucial question is, how he arrived at, what suggested, the doctrine of combination of atoms in multiple proportion?

Two main accounts of the origin of the theory have

[8] *Nicholson's Jour.*, vol. 29, p. 147, 1811; see also "New System of Chemical Philosophy," vol. 1, p. 216, 1808.

been offered. They have already been mentioned in the second paper of this series. The first of these, coming direct from Thomas Thomson, is that Dalton discovered the composition of marsh gas and olefiant gas and was led thereupon to perceive the law of multiple proportions, and to devise his chemical theory as an explanation of the law. This may be called the *inductive* account.

Again, Roscoe and Harden accept an account, offered by Dalton, which may be called the *deductive* one. Dalton had formed his diffusion hypothesis without considering the "effect of *difference of size* in the particles of elastic fluids." On consideration he found that "the *sizes must* be different," and thereupon he revised his diffusion theory. He then introduces the subject of the chemical theory :— "The different *sizes* of the particles of elastic fluids .... being once established, it became an object to determine the relative *sizes* and *weights*, together with the relative *number* of atoms in a given volume. This led the way to the combination of gases," etc.

Objections to the purely inductive and deductive accounts.

There being these two accounts, the inductive one and the deductive, of the origin of the theory, there arises the question, which comes nearer the truth? The Board of Education has recently committed itself to an opinion on this topic, in the course of its criticisms on the answers of students to its questions on chemistry. The particular question was :—"Give a short account of Dalton's atomic theory, and discuss its value in explaining the laws of chemical combination."

Teachers of chemistry, to judge from the reference made to them, have been adopting Roscoe and

Harden's view of the matter. "... The teachers are to blame ... in allowing so many of their students to put the "cart before the horse" as they do in connection with the atomic theory. The idea seems to prevail that the laws of chemical combination follow from the atomic theory, whereas the laws of combination were established first as the results of experiments, and the atomic theory of Dalton provides an explanation of the facts."[9]

It is, of course, begging the question to assume that the matter is as simple as this. Everyone knows which is the cart and which is the horse, and no one knows for certain how Dalton's chemical theory arose. Again, one may urge, that supposing the origin of the theory to be a controversial matter, the Board of Education is not called upon to take one side or the other, and indeed, might well avoid such topics in its examination papers.

The matter, however, is no longer controversial, being so far settled that the purely inductive view of the origin is quite untenable. There is the objection to it in principle, that it says nothing about Dalton's physical theory to which W. C. Henry drew attention long ago, and Roscoe and Harden recently. Besides, Roscoe and Harden have advanced objections to it in detail, which must be final to anyone who considers them.[10]

Reasons must now be offered for rejecting the deductive account which Roscoe and Harden have accepted. The gist of it is that Dalton *first* satisfied himself that the atoms of different gases have different sizes, and *then* devised the chemical theory. This, Dalton's own narrative, has already been quoted on p. 10. He gave it seven years after the events which it relates, and it is quite unsatisfactory. It does not condescend to

[9] "Science Examinations," 1909, p. 119. Board of Education.

[10] "New View of the Origin of Dalton's Atomic Theory," p. 28.

particulars and instances. Dalton does not explain, nor is it obvious that anyone can explain, how he was to test the sizes of atoms without some kind of chemical theory. One may either *assume* that different atoms have the same size, and act accordingly, or one can endeavour to test the position, by obtaining data regarding atoms, on the basis of some hypothesis as to the way in which they combine chemically.

It has been shown in this paper that Dalton, so far as the formation of the chemical theory is concerned, did not act on the belief that atoms of different kinds have the same size. Again, the author has already shown, in the paper on Dalton's physical atomic theory, that the chemical theory was formed first and the conclusion that "atoms" of different gases were different in size was come to afterwards.

This is the order that was to be expected in the nature of the case. Moreover, there is nothing in the note-books to show that the chemical theory was devised except for its own sake. The testing of the sizes of atoms was an afterthought. The connection between the sizes of atoms and the diffusion of gases was not considered till a year after the chemical theory had been formed.

The experiments of August 4th, 1803.

The chief matter that continues to be doubtful is the exact way in which Dalton arrived at the law of multiple proportion. The author, after a careful consideration of the evidence, can come to no other conclusion than that it was Dalton's experiments on the combination of nitric oxide and oxygen that aroused his attention, and made him apply his physical theory to the purposes of chemistry.

The facts, as established by the note-books, are that Dalton, for the purpose of his inquiry into the composition of the atmosphere, was studying the combination of nitric oxide and oxygen in the year 1803. He was at work on the subject during March and April, and then again in August. On the 4th of August he obtained the well-known result that 100 measures of air could take 36, or 72, of nitric oxide.[11] His first table of atomic weights was drawn up by the 6th of September.

The first case of combination in multiple proportions observed by Dalton must have seemed of great importance to him. His observation of August 4th, regarding nitric oxide and the oxygen of the air, is the first of the kind which he recorded. It is difficult to suppose that he can have known an earlier one. Yet Roscoe and Harden think that this case was of comparative unimportance in the development of the atomic theory. Their reason is that the chemical compounds concerned are not sufficiently represented in the first table of atomic weights. The chemical changes, as Dalton understood them, may be set out in the equations :—

(1) $NO + O = NO_2$ (nitric acid).
(2) $2NO + O = N_2O_3$ (nitrous „).

Certainly, if the whole matter turned on nitrous acid, Roscoe and Harden argue, it is surprising that Dalton ignored this substance in making up his table on September 6th. They suggest that the symbol for nitrous acid which appears at the side of the table was added afterwards, probably about the 12th October. Everyone must admit this who inspects the original table, or the photograph in Roscoe and Harden's book.

Dalton seems to have set aside the case of nitrous acid

[11] Roscoe and Harden, *op. cit.*, pp. 34, 38.

for a time as being too complicated. The union of two atoms of one kind with three of another must have appeared at that stage of thought to be very complex. Dalton did not adopt such a formula till October. On the 12th of that month, as a summary of his views, he gives tables of binary compounds, of ternary, of compounds of 4 atoms, and compounds of 5. Alcohol and nitrous acids were the only compounds of 5 atoms. Alcohol is ether and water united, or 2 oxygen, 2 carbon, and 1 hydrogen. Nitrous acid is 3 oxygen and 2 nitrogen.

The objection of Roscoe and Harden, however, must be final, but for one circumstance: the objection ignores the physical theory. The experiments with nitric oxide and air must have received lengthy consideration had it not been for the fact that Dalton had an atomic theory already in his mind. As it was, these experiments simply served to give the impulse needed to set his mind working. Under that stimulus he made a beginning with the adaptation of the physical theory to chemical purposes.

Nothing more was needed. Larmor, in his *Wilde Lecture* on the "Physical Aspect of the Atomic Theory," represents that the doctrine of combination of atoms in the proportion 1 : 1 must forthwith lead to other cases such as 1 : 2.

"Once it is postulated that only one kind of aggregation into molecules occurs, *e.g.*, that in water there is only one way in which the hydrogen attaches itself to the oxygen, the laws of definite and multiple proportions are self-evident."[12]

Earlier in this paper, the author has pointed out how the doctrine of 1 : 1 arose logically from the physical theory. There are here, therefore, all the elements of a fair account of the origin of Dalton's chemical theory.

[12] *Manchester Memoirs*, vol. 52, no. 10, p. 9. 1908.

The germ of it is to be found in Newton's theory and in Dalton's physical theory of the year 1801, and one must recognise the space of two years during which it remained in the germ. There comes then the experiment of the 4th of August, 1803, sufficient to arouse Dalton's attention and make him apply his theory to the purposes of chemistry. He frames the rule of 1 : 1, then considers the less simple cases, and tests his ideas by the available analytical data. By the 6th of September he is able to draw up the first atomic-weight table.

Chemistry without the atomic theory.

Attempts have been made in recent years, by Wald and Ostwald, to deduce the laws of chemical combination from first principles, without making any use of the atomic theory.[13] It seems to the author worth pointing out here that there is no connection between the modes of thought taken by these writers, and the process by which these laws were actually established. With the atomic theory as a starting point, they were formulated by Dalton and completely established by Berzelius. Moreover, at the same time and as a matter of course, the foundations of chemical analysis as a genuine science were laid.

The failure of other workers.

Sufficient attention has not been given to the question, why it should have been left to Dalton to draw attention to the law of multiple proportion? It was not the want of interest in the subject of chemical composition. The workers on the subject, towards the end of the eighteenth and the beginning of the nineteenth century, were quite numerous. One may name Bergman, Wenzel, Klaproth, Lavoisier, Richter, Kirwan, Thomson, Bucholz, Chenevix,

[13] See the Faraday Lecture, *Trans. Chem. Soc.*, 1904.

Bostock, Clément, Désormes and Proust. Yet the failure of these chemists to discover the law of multiple proportion, despite their immense labours, was complete.

An incorrect explanation of the failure.

The reason usually offered for this failure is, that the data for the composition of substances were calculated in such a way as to hide the law.[14] Plainly the implication is, that the data calculated in a suitable way must reveal the law at once. This is mere guess-work, for as a matter of fact, data were frequently stated in precisely the way required. Proust, for instance, gives practically all his data for the oxides and sulphides of a metal, in terms of 100 parts of the metal.[15]

The true explanation.

The true explanation is twofold. In the first place, accurate chemical analysis is impossible without a check of some kind. That the analyst should have good intentions, even the best intentions, is not enough. In the absence of a guiding principle, chemists cannot tell when a substance is pure, or when an analysis is correct. As explained in the first paper of this series, it was this state of uncertainty which contributed at the beginning of the nineteenth century, more than anything else, to the spread of C. L. Berthollet's ideas regarding combination in indefinite proportions. Arrhenius has pointed out that every chemist *now* prepares his substances so that

[14] E. von Meyer, "Hist. of Chem.," Eng. trans., pp. 195-196, 1906, and Arrhenius, "Theories of Chem.," Eng. trans., p. 16, 1907.

[15] *Ann. de Chim.*, vol. 28, p. 214, 1798; *Jour. de Phys.*, vol. 54, p. 92, 1802; vol. 55, p. 330; vol. 59, p. 324, 326, 330, 352, 1804; vol. 62, p. 136, 138, 139, 1806: vol. 63, p. 431, 1806.

they agree with the laws of definite and multiple proportions.

In the second place Dalton was at an advantage over other workers, in having a theory to which he could refer facts. Something more is needed than important facts, one must have the eye to perceive their importance. Charles Darwin gives an illustration of this when he admits he once walked along a valley, full of the plainest indications of glacial action which he absolutely failed to notice. "On this tour I had a striking instance how easy it is to overlook phenomena, however conspicuous, before they have been observed by anyone. We spent many hours in Cwm Idwal, examining all the rocks with extreme care, as Sidgwick was anxious to find fossils in them; but neither of us saw a trace of the wonderful glacial phenomena all around us; we did not notice the plainly scored rocks, the perched boulders, the lateral and terminal moraines. Yet these phenomena are so conspicuous that, as I declared many years afterwards—a house burnt down by fire did not tell its story more plainly than did this valley."[16]

This is not a fanciful argument, but one that can be amply justified by facts. Chemists did not go on making analyses conscientiously without sometimes obtaining data in good agreement with the law of multiple proportion. But they quite failed to perceive the significance of the data. Dalton himself was able afterwards triumphantly to point out more than one such case, which had escaped the notice of the chemist concerned. He quotes Bostock's analyses of the acetate and superacetate of lead:—[17]

[16] "Life and Letters of Charles Darwin," 3 vols., 1887, vol. 1, p. 57.

[17] *Nicholson's Jour.*, vol. 11, p. 75, 1805; vol. 29, p. 150, 1811.

	Acetate.	Superacetate.
Lead	100	100
Acid	24	49

Again, he gives the instance of the oxides of carbon: "Carbonic oxide contains just half the oxygen that carbonic acid does, which indeed had been determined by Clément and Désormes . . . who, however, had not taken any notice of this remarkable result." [18]

[18] Roscoe and Harden, *op. cit.*, p. 117.

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VII. The Behaviour of Bodies floating in a Free or a Forced Vortex.

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§ 1. To anyone who has watched the behaviour of bodies floating in a vortex, whether of dimensions comparable with that of the whirlpool in the Niagara Gorge or such an one as may be formed in stirring one's tea, and who has noted how some objects are apparently irresistibly drawn into the centre of the vortex, while others revolve around its outer boundary, and others again alternately approach and recede from its centre, it must be apparent that the forces producing these various results must be of considerable complexity.

In a series of experiments recently carried out by the author an attempt has been made to determine how, in either a free or a forced vortex, the behaviour of the object depends upon :—

(*a*) Its size, the depth of immersion remaining constant.

(*b*) The linear dimensions, in similar objects of the same specific gravity.

(*c*) The depth of immersion, in bodies of the same cross sectional area but of different specific gravities.

(*d*) The position of the centre of gravity in non-homogeneous bodies of the same size and shape.

(*e*) The shape of the body.

(*f*) The intensity of the vortex action.

March 7th, 1911.

§ 2. *Experiments on Free Vortex.* The free vortex experiments were carried out in a cylindrical tank, two feet in diameter and one foot deep. This is supplied with water through a pipe 1½in. in diameter, making connection with the tank through an external volute whose centre line is six inches above the bottom of the tank, while discharge takes place through a central hole in the bottom of the tank. The intensity of the vortex action was varied by enlarging this hole from one inch in the first series of experiments to 1½in. in the second series, and by varying the head of water in the tank from 9 inches to 12 inches. Throughout the experiments the motion approximated very sensibly to that of flow in a true free vortex. The motion, as investigated by colour bands, was steady and non-sinuous, and the surface smooth and free from waves.

In the experiments carried out under a head of nine inches, the form of the surface profile is indicated in the following table.

Radius (ins.) ...	12	10	8	6	4	3	2·5	2·0	1·5	1·0	·5
Depth of surface below surface level at outer circumference of tank, in inches, with 1 inch orifice	·00	·025	·050	·085	·13	·19	·27	·40	·64	1·14	2·10
Ditto, with 1½ inch orifice	·00	·030	·060	·13	·26	·43	·67	1·06	1·61	2·96	—

The first of these orifices discharges ·0109 cub. ft. per second, and the second ·0152 cub. ft. per second under this same head.

A series of experiments carried out to determine the value of the coefficient of discharge for each of the orifices

under heads varying from 8 to 12 inches, showed this to be sensibly independent of the head, and to have value of ·287 in the 1in. orifice and ·178 in the 1½in. orifice.

The accompanying tables detail the behaviour of the floating bodies in typical cases of the experiments of each series.

The following appear to be the main conclusions to be drawn from the free vortex experiments.

(*a*) Floating particles whose dimensions are very small compared with those of the orifice, rotate in spiral paths approaching with a continually increasing velocity, and finally disappearing down the funnel of the vortex. The rate of approach of such particles is sensibly the same as that of the fluid itself. (In the second series of experiments such particles, of sawdust, described about 40 revolutions while approaching the centre from 9 inches radius.) The lighter particles, however, show a distinct tendency to approach the centre more rapidly than those of a higher specific gravity.

(*b*) If of dimensions which are moderate compared with those of the vortex, the behaviour depends largely on the shape, size, weight, and position of the centre of gravity of the object. In every case the latter rotates, about its own axis, relative to the surrounding water, in the opposite direction to that of its revolution around the centre of the vortex. If introduced near the periphery of the vessel it usually approaches the centre, and may either settle down to rotate in seeming equilibrium at some definite radius, alternately approach and recede from the centre, or straightway disappear down the funnel.

TABLE A.—**Bodies in free vortex formed by water discharging from a central hole, 1″ diameter, in the bottom of a circular tank 2′ 0″ diameter under a head of 9 inches.**

Experiment.	Form of object.	Dimensions.	Behaviour.
1	Circular cardboard disc ...	3″ diam. × $\frac{1}{32}$″ thick	Put in at 9″ radius. Settles down to describe circle, radius from $3\frac{7}{8}$″ to $4\frac{1}{8}$″. Makes 1 rotation in direction of revolution in 5 revs., *i.e.*, makes four rotations relative to water in opposite direction to revolution, while making 5 revs. If displaced slightly inwards is thrown outwards to $7\frac{1}{2}$″ radius.
2	,,	2″ ,, × $\frac{1}{32}$″ ,,	Circles at 3″ to $3\frac{1}{4}$″ radius. Makes 1 rotation in same direction in $3\frac{5}{8}$ revs. If displaced slightly inwards from 3″ circle, is thrown outwards to $3\frac{3}{4}$″ radius.
3	,,	$1\frac{1}{2}$″ ,, × $\frac{1}{32}$″ ,,	Circles at $2\frac{3}{8}$″ to $2\frac{7}{8}$″ radius. Very stable as regards inward displacements. Thrown out from $2\frac{1}{4}$″ to $3\frac{3}{4}$″ radius.
4	,,	1″ ,, × $\frac{1}{32}$″ ,,	Circles at about 7″ radius. If displaced inwards settles down to circle at $2\frac{1}{8}$″ to $2\frac{1}{4}$″ radius again. If displaced inwards from this is drawn down funnel.
5	,,	$\frac{3}{4}$″ ,, × $\frac{1}{32}$″ ,,	Circles at about $7\frac{1}{2}$″ radius. If displaced inwards approaches and goes down funnel. Makes 1 rotation in $2\frac{5}{8}$ revs.
6	,,	$\frac{1}{2}$″ ,, × $\frac{1}{32}$″ ,,	Circles at $10\frac{1}{2}$″ to 11″ radius. If displaced inwards is drawn down funnel. Makes 1 rotation in 2 revs.
7	Circular wooden cylinder, specific gravity ·6, floating with axis vertical.	2″ diam. × $1\frac{1}{2}$″ long	Put in at 9″ radius. Disappears after $6\frac{1}{4}$ revs.
8	,,	2″ ,, × 1″ ,,	Put in at 9″ radius. Disappears after 7 revs.
9	,,	2″ ,, × $\frac{1}{4}$″ ,,	Arrives at $2\frac{3}{8}$″ radius in 4 revs. Circles at this radius unless displaced.
10	,,	$1\frac{1}{2}$″ ,, × 1″ ,,	Circles at $7\frac{1}{4}$″ radius. If put in at 7″ radius is drawn down in 3 revs. In one experiment placed in at 9″ radius made 13 revs. before being drawn down.

13	,,	1″ ,, × $\frac{1}{4}$″ ,,	,, $7\frac{1}{4}$″ ,, ,, 6″ ,,	9 ,,
14	,,	1″ ,, × $\frac{1}{8}$″ ,,	,, $7\frac{1}{4}$″ ,, ,, 6″ ,,	16 ,,
15	,,	1″ ,, × $\frac{1}{2}$″ ,,	,, 8″ ,, ,, 6″ ,,	$10\frac{1}{2}$,,
16	Ditto of cork. Sp. gravity ·4	1″ ,, × $\frac{3}{4}$″ ,,	,, $7\frac{1}{2}$″ ,, ,, 6″ ,,	$9\frac{1}{3}$,,
17	Wooden cylinder	4″ ,, × 1″ ,,	,, $7\frac{1}{4}$″ ,, ,, 6″ ,,	7 ,,
			Put in at 9″ radius, is drawn down funnel in $\frac{3}{4}$ revs.	
18	Short cylinder (cork), axis vertical, immersed $\frac{3}{8}$″	$1\frac{1}{2}$″ diam. × 1″ long	Put in at 9″ radius, is drawn down funnel in $5\frac{2}{3}$ revs.	
19	Ditto, loaded so as to keep C.G. central. Sp. gravity unity.	$1\frac{1}{2}$″ ,, × 1″ ,,	Appears stable at about $6\frac{7}{8}$″ radius. If put in at 6″ radius is down in 4 revs.	
20	Ditto, loaded only on bottom. Sp. gravity unity.	$1\frac{1}{2}$″ ,, × 1″ ,,	Stable at $6\frac{5}{8}$″ radius. If put in at 6″ radius gets to 2″ radius in 5 revs. and is then thrown out to 6″ radius. This repeated indefinitely, except that occasionally the throw out carries it beyond the critical radius $6\frac{5}{8}$″ when body does not approach nearer centre. Usually makes 4 to 6 revs. while oscillating to and from centre. At instant of being thrown out is rotated rapidly in direction of revolution, but this motion dies down quickly.	
21	Short wooden cylinder, loaded equally top and bottom. Sp. gravity unity.	2″ ,, × $1\frac{1}{2}$″ ,,	Circles at 8″ radius. If put in at 7″ radius is down in 5 revs. If at 6″ down in 2 revs.	
22	Ditto, but loaded on bottom face only so as to give immersion of $1\frac{1}{8}$″.	2″ ,, × $1\frac{1}{2}$″ ,,	Circles at $7\frac{1}{4}$″ radius. If put in at 7″ radius is down in 4 revs.	
23	Square wooden disc. Sp. gravity ·6.	2″ square × 1″ thick	Put in at 9″ radius. Drawn down in 2 revs.	
24	,,	$1\frac{1}{2}$″ ,, × $\frac{1}{4}$″ ,,	,, 9″ ,, ,, 3 ,,	
25	,,	1″ ,, × $\frac{1}{4}$″ ,,	Circles at about $7\frac{1}{2}$″ radius. Put in at 6″ radius, drawn down in 6 revs.	
26	,,	$\frac{1}{2}$″ ,, × $\frac{1}{4}$″ ,,	,, 8″ ,, If displaced inwards rapidly approaches to $3\frac{1}{8}$″ radius. Drawn down from this radius in about 40 revs.	

TABLE A.—*Continued.*

Experiment.	Form of object.	Dimensions.	Behaviour.
27	Square cardboard disc ..	3″ square × $\frac{1}{32}$″ thick	Circles at $4\frac{3}{8}$″ to $4\frac{5}{8}$″ radius. Makes one rotation in 4 revs. Is drawn down if displaced inwards.
28	,,	2″ ,, × $\frac{1}{32}$″ ,,	Circles at $2\frac{7}{8}$″ to $3\frac{1}{4}$″ radius. Makes 1 rotation in $4\frac{7}{8}$ revs.
29	,,	$1\frac{1}{2}$″ ,, × $\frac{1}{32}$″ ,,	,, $2\frac{5}{8}$″ to $2\frac{3}{4}$″ ,, ,, 1 ,, 5 ,,
30	,,	1″ ,, × $\frac{1}{32}$″ ,,	,, 9″ to 11″ ,, ,, 1 ,, 2 ,,
31	,,	$\frac{3}{4}$″ ,, × $\frac{1}{32}$″ ,,	,, 9″ to 11″ ,, ,, 1 ,, $2\frac{7}{8}$,,
32	,,	$\frac{1}{2}$″ ,, × $\frac{1}{32}$″ ,,	,, 9″ to 11″ ,, ,, 1 ,, $2\frac{1}{2}$,,
			All these discs are drawn down funnel if displaced inwards from equilibrium circle. In all square and rectangular discs, as body revolves each vertex in turn is distinctly attracted towards centre so that it rotates in a series of jerks, always dwelling for a short time with a diagonal radial.
33	Rectangular cardboard disc.	3″ long × 1″ wide	Fairly stable at 3″ to $3\frac{1}{2}$″ radius. If waterlogged and put in at 9″ radius is drawn down in 2 revs.
34	,,	2″ ,, × 1″ ,,	Circles at 10″ radius. If put in at 9in radius is drawn down in 7 revs.
35	,,	$1\frac{1}{2}$″ ,, × 1″ ,,	Circles at 2″ to $2\frac{1}{4}$″ radius—also at 3″ to 10″ radius.
36	,,	3″ ,, × $\frac{1}{2}$″ ,,	Unstable at all radii. Put in at 9″ radius arrives at centre in 5 revs. Does not actually disappear, but rotates with its upper end on lip of funnel.
37	,,	$1\frac{1}{2}$″ ,, × $\frac{1}{2}$″ ,,	Circles at 8″ to 10″ radius. Put in at 7″ radius is down in 5 revs.
38	,,	1″ ,, × $\frac{1}{2}$″ ,,	Circles at 6″ to 9″ radius.
39	Rectangular wooden block (immersed $\frac{5}{8}$″).	$3\frac{1}{4}$″ long × $1\frac{1}{2}$″ wide × $1\frac{1}{4}$″ deep	Put in at 9″ radius is drawn down in $1\frac{1}{4}$ revs.
40	Ditto of oak (immersed $1\frac{1}{8}$″).	,,	,, 9″ ,, 2 ,,
41	Ditto of deal, loaded on bottom to give same ($1\frac{1}{8}$″) immersion.	,,	,, 9″ ,, $2\frac{1}{2}$,,

43	Circular cork cylinder, loaded at one end so as to float vertically just immersed.	2″ long × $\frac{3}{4}$″ diam.	Appears to be in neutral equilibrium at any radius from 7″ to 10″. If put in at any radius from $2\frac{1}{2}$″ to 7″ gradually works out to 7″ again. If put in at less radius than $2\frac{1}{2}$″ is drawn down vortex.
44	,,	1″ ,, × $\frac{3}{4}$″ ,,	,, ,, ,,
45	Wooden cylinder, loaded as above.	$3\frac{1}{4}$″ ,, × $\frac{3}{4}$″ ,,	,, ,, ,,
46	Cylinder (of cork), axis horizontal.	2″ long × $\frac{3}{4}$″ diam.	Put in at 9″ radius is drawn down in $3\frac{1}{2}$ revs.
47	Ditto of wood, specific gravity ·6.	2″ ,, × $\frac{3}{4}$″ ,,	,, 9″ ,, 4 ,,
48	Ditto, evenly loaded so as to be submerged.	2″ ,, × $\frac{3}{4}$″ ,,	,, 9″ ,, $4\frac{1}{2}$,,
49	Ditto of wood, unloaded ...	$3\frac{1}{4}$″ ,, × $\frac{3}{4}$″ ,,	,, 9″ ,, $1\frac{3}{4}$,,
50	,,	$4\frac{1}{4}$″ ,, × $\frac{3}{4}$″ ,,	,, 9″ ,, $1\frac{3}{8}$,,

TABLE B.—**Vortex discharging through 1″ hole under 12″ head.**

1	Circular cardboard disc ...	3″ diam. × $\frac{1}{32}$″ thick	Circles at $4\frac{1}{4}$″ to $4\frac{1}{2}$″ radius. If displaced inwards slightly is thrown out to $7\frac{1}{2}$″ radius.
2	,, ...	2″ ,, × $\frac{1}{32}$″ ,,	Circles at $3\frac{3}{8}$″ radius. Makes 1 rotation in $2\frac{3}{8}$ revs.
3	,, ...	$1\frac{1}{2}$″ ,, × $\frac{1}{32}$″ ,,	,, $2\frac{7}{8}$,, ,, 1 ,, $3\frac{5}{8}$,,
4	,, ...	1″ ,, × $\frac{1}{32}$″ ,,	Circles at $7\frac{1}{4}$″ radius and again at $2\frac{1}{4}$″ radius. If waterlogged is stable at $2\frac{1}{4}$″ radius, revolving below surface with its plane tangential to its path and inclined at about 45° to horizontal, its upper edge being nearer centre of vortex.
5	,, ...	$\frac{3}{4}$″ ,, × $\frac{1}{32}$″ ,,	Circles at 7″ to 9″ radius.

TABLE C.—Vortex formed by water discharging through $1\frac{1}{2}''$ hole under a head of 9 inches.

Experiment.	Form of object.	Dimensions.	Behaviour.
1	Cylindrical cardboard disc	$3''$ diam. × $\frac{1}{32}''$ thick	Circles at $4\frac{1}{4}''$ radius. If displaced slightly inwards is thrown outwards.
2	,,	$2''$,, × $\frac{1}{32}''$,,	,, $4''$,, Makes 1 rotation in 3 revs.
3	,,	$1\frac{1}{2}''$,, × $\frac{1}{32}''$,,	In unstable equilibrium at $2\frac{1}{4}''$ radius. Approaches from $9''$ radius in 1 rev.
4	,,	$1''$,, × $\frac{1}{32}''$,,	Put in at $9''$ radius approaches centre steadily and goes down in 25 revs.
5	,,	$\frac{3}{4}''$,, × $\frac{1}{32}''$,,	,, $9''$,, ,, ,,
6	Wooden cylinder. Sp. gravity ·6	$2''$ diam. × $1\frac{1}{2}''$ long	Put in at $9''$ radius, disappears in 4 revs.
7	Axis vertical	$2''$,, × $1''$,,	,, $9''$,, $4\frac{3}{4}$ revs.
8	,,	$2''$,, × $\frac{1}{4}''$,,	Approaches quickly to $2\frac{1}{4}''$ radius and then takes 15 revs. to disappear.
9	,,	$1\frac{1}{2}''$,, × $1''$,,	Circles at $8\frac{1}{2}''$ radius. Put in at $7''$ radius is drawn down in 7 revs.
10	Ditto, loaded top and bottom. Sp. gravity unity	$1\frac{1}{2}''$,, × $1''$,,	,, $8''$,, Put in at $5''$ radius works out to $8''$. Put in at $4''$ disappears down vortex.
11	Cork cyl., loaded on bottom. Sp. gravity unity	$1\frac{1}{2}''$,, × $1''$,,	Circles at $7''$ radius. Put in at $6''$ radius is drawn in to $2''$ radius, then thrown out to $6''$ and so on.
12	Cork cylinder	$3''$,, × $\frac{3}{4}''$ thick	Put in at $9''$ radius, disappears in 1 rev.
13	,,	$1''$,, × $\frac{1}{4}''$,,	,, $9''$,, ,, $2\frac{1}{2}$ revs.
14	Square cardboard disc	$3''$ square × $\frac{1}{32}''$ thick	Put in at $9''$ radius, circles at $4\frac{5}{8}''$ radius } All drawn down vortex if displaced inwards from equilibrium circle.
15	,,	$2''$,, × $\frac{1}{32}''$,,	,, $9''$,, $3\frac{3}{8}''$,, }
16	,,	$1\frac{1}{2}''$,, × $\frac{1}{32}''$,,	,, $9''$,, $3''$,, }
17	,,	$1''$,, × $\frac{1}{32}''$,,	,, $9''$ radius, approaches centre quickly and makes 35 revs. at $1\frac{1}{2}''$ radius before vanishing.

18	Rectangular cardboard disc	3″ long × 1″ wide	Put in at 9″ radius, disappears in 4 revs.
19	,,	2″ ,, × 1″ ,,	,, 9″ ,, ,, 5½ ,,
20	,,	1½″ ,, × 1″ ,,	,, 9″ ,, down in 9 ,,
21	,,	3″ ,, × ½″ ,,	,, 9″ ,, ,, 2½ ,,
22	,,	2″ ,, × ½″ ,,	,, 9″ ,, ,, 5 ,,
23	,,	1½″ ,, × ½″ ,,	,, 9″ ,, ,, 8 ,,
24	Square wooden disc. Sp. gravity ·6	2″ square × 1″ thick	Put in at 9″ radius, down in 2½ revs.
25	,,	2″ ,, × ⅜″ ,,	,, 9″ ,, ,, 3 ,,
26	,,	2″ ,, × ¼″ ,,	,, 9″ ,, ,, 3½ ,,
27	,,	1″ ,, × ¼″ ,,	,, 9″ ,, ,, 33 ,,
28	,,	½″ ,, × ¼″ ,,	Circles at 7¾″ radius and again at 5″ radius. If displaced inwards from 5″ radius, quickly vanishes.
29	Ditto, loaded on bottom. Sp. gravity unity	2″ ,, × 1″ ,,	Circles at 7¼″ radius. Put in at 6″ radius is down in 13 revs.
30	Rectangular wooden block, immersed ⅝″	3¼″ long × 1½″ wide × 1¼″ deep	Put in at 9″ radius, down in 1 rev.
31	Ditto, of oak, immersed 1⅛″	,,	,, 9″ ,, ,, 1⅞ ,,
32	Ditto, of deal, bottom loaded to give 1⅛″ immersion	,,	,, 9″ ,, ,, 2 ,,
33	Wooden cylinder, axis horizontal	4¼″ long × ¾″ diam.	Put in at 9″ radius, down in 1⅛ revs.
34	Ditto	3¼″ ,, × ¾″ ,,	,, 9″ ,, ,, 1⅝ ,,
35	Ditto, loaded evenly until Sp. gravity is unity	3¼″ ,, × ¾″ ,,	,, 9″ ,, ,, 2¼ ,,
36	Ditto, loaded at one end to float vertically with Sp. gravity unity	3¼″ ,, × ¾″ ,,	Circles at 11″ radius. Put in at 4½″ radius works out to 11″. Put in at 4″ radius, is drawn down vortex.

(1) Where homogeneous bodies have the same specific gravity, depth of immersion, and shape of plane of flotation, generally speaking the larger shows the greater tendency to approach the centre. In bodies, the section of whose plane of flotation approximates to a rectangular form, this appears to be generally true, but in circular cylindrical bodies floating with vertical axes, there appears to be a critical diameter,—from 1in. to 1½in. in these experiments—for which the repellent effect of the vortex for small radii of revolution is very marked. Objects, whether of a greater or less diameter than this, show a greater tendency to be drawn into the vortex, though this effect is more marked with increasing than with diminishing sizes. Cf. Experiments A (1 to 10); B (1 to 5); C (1 to 9). The same applies, in a lesser degree in the case of bodies of square section, but here the critical size appears to be somewhat less. Cf. A (23 to 26); C (24 to 28); A (27 to 32); C (14 to 17). Cylindrical objects of a size somewhat larger than, but approximating to the critical, appear to have a definite circle of rotation on the lip of the funnel, in which, except when affected by extraneous circumstances, they may rotate indefinitely. If displaced outwards from this circle they return, while if displaced inwards they are drawn down the funnel. Objects somewhat smaller than the critical size—from ½in. to 1in. diameter—have an equilibrium circle of much greater radius, usually from 7 to 10 inches in these experiments. The radius of the equilibrium circle for a given object increases with the intensity of the vortex. For objects of greater size than the critical, it increases, within limits, with the size of object, the largest object to have an equilibrium circle in these experiments being of three inches diameter.

(2) Where similar homogeneous bodies are of the same specific gravity, the larger tends to approach the centre more rapidly. Cf. A (16 & 17), (8 & 14); C (12 & 13).

(3) In bodies of the same shape and size but of different specific gravities the lighter tends to approach the centre more rapidly. Cf. A (12 & 16), (18 & 19), (7 & 21), (39 & 40), (46, 47 & 48); C (9 & 10), (30 & 31), (34 & 35).

(4) In non-homogeneous bodies of the same size, shape and weight, the lower the centre of gravity the less is the tendency to approach the centre. With the C.G. sufficiently low down the body gradually works out from the centre of the vortex. Cf. A (19 & 20), (40 & 41), (43, 44 & 45); C (10 & 11), (31 & 32), (35 & 36). Comparing A (21 & 22) it appears that the relative lightness of cylinder 22 more than counterbalances the change in the relative position of the centre of gravity as compared with cylinder 21.

In homogeneous bodies of the same size and depth of immersion, those more nearly approximating to a circular form of cross section show the lesser tendency to approach the centre, the difference becoming more marked as the size increases. Cf. A (1 to 6, 27 to 32 & 33 to 38); (8 & 23), (12 & 25), (23 & 39); C (1 to 5, 14 to 17 & 18 to 23); (24 & 30); (7 & 24).

In vortices whose intensity is increased by increasing the quantity of water discharged, either by increasing the head or the size of the discharge orifice, the observed phenomena are intensified. For bodies revolving in equilibrium, the equilibrium circle is larger in the stronger vortex, while the minimum size of body capable of revolving in equilibrium increases with the strength of vortex. Cf. tables A, B, and C.

§ 3. A consideration of the forces acting on a body floating in a free vortex affords an explanation of the seemingly anomalous nature of these results.

The resultant force is due to :—

(1) The pressure of the surrounding water. Apart from any relative motion of body and water, this would have a resultant normal to the surface and the body would approach the centre at the same rate as the contiguous filaments.

(2) The modification introduced by centrifugal action. In a homogeneous body of the same specific gravity as water the centrifugal force being the same as would be exerted on the displaced fluid has no tendency to produce radial motion. If, however, the centre of gravity of the object is below its centre of buoyancy, then on account of the inclined position of the rotating body its effective radius of rotation is greater and this force tends to cause outward motion. If the C.G. is higher than the centre of buoyancy the force tends to produce inward motion.

Thus, due to this cause alone, a light homogeneous body tends to approach the centre more rapidly than a similar, but heavier body, and the latter more rapidly than a non-homogeneous body of the same shape, size, and weight. This effect will always be more marked the greater the distance between the C.G. and the centre of buoyancy, and hence, in a light homogeneous body, the greater the vertical height of the body, so that in such bodies of the same shape and cross-sectional area, the one having the largest vertical

dimensions tends to approach the centre most rapidly.

If W lbs. be the weight of the body; r the radius of the path of its C.G.; r_1 that of the path of the centre of buoyancy; v the velocity of the mass-centre and v_1 of the centre of buoyancy, then at a given instant

$$v_1 = \frac{vr_1}{r},$$

and the resultant radial outward force acting in virtue of centrifugal action is given by

$$\frac{W}{g}\left\{\frac{v^2}{r} - \frac{v_1^2}{r_1}\right\} = \frac{W}{g}v^2\left\{\frac{r - r_1}{r^2}\right\} \text{lbs.}$$

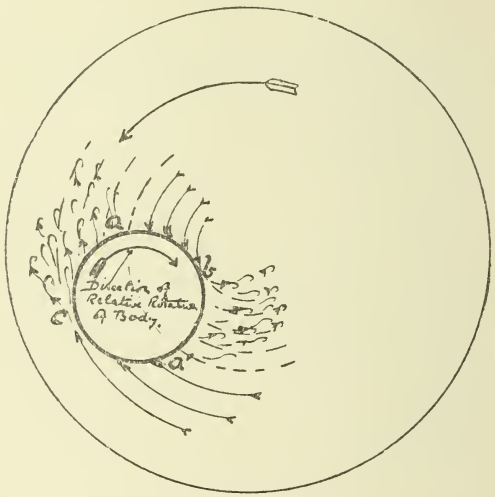
Since in a free vortex $v \propto \frac{1}{r}$, this force becomes directly proportional to $r - r_1$, and inversely proportional to r^4. Owing to the increasing inclination of the body as the centre is approached $r - r_1$ increases as r diminishes, so that this effect varies inversely as a higher power of the radius than the fourth.

If the intensity of the vortex be defined as the velocity at a given radius, this action will evidently vary as the square of the intensity.

(3) The fact that, since the velocity of the water varies inversely as the distance from the centre, there is a relative motion of water and solid, in the direction of revolution, over that portion of the periphery marked *aba'* in the *Figure*, and, in the opposite direction, over the periphery marked *a'ca*. This causes the body to rotate about its own axis *relatively to the surrounding water*, in the *opposite* direction to that of its revolution around the

centre of the vortex. Since, however, in virtue of its revolution, the object makes one rotation per revolution in the direction of revolution, the *absolute* direction of rotation will usually be in the same direction to that of revolution.

This rotation, relative to the surrounding water, has indirectly, an important bearing on the behaviour of the



object. To realise this it must be remembered that, other effects neglected, the body tends to gravitate to the centre of the vortex, in virtue of the inward spiral flow. But over the periphery *aba'* (*Fig.*) the velocity of the surrounding fluid is greater than that of the body, and in virtue of the rotation of the body, this flow will be deflected *inwards* towards the centre of the vortex. Consequently the force accompanying the change of momentum of this passing stream of water will have an *outward* radial component. Similarly, since the portion *aca'* of the body is moving past its contiguous fluid this

portion of the wake will be deflected *outwards* by the rotation,* and the corresponding force on the body will, in consequence, have an *inward* radial component. As, however, the relative velocity is greater over the portions of the body nearer the centre of the vortex, the resultant force will have an *outward* radial component. The relative magnitude of this is likely to be greater the larger the diameter of the body, and the greater its depth of immersion; and, for bodies of different shapes, is likely to be least where eddy formation in the rear of the body is least marked, and therefore, with bodies of a more or less circular cross section.

Since the magnitude of these forces will vary approximately as the square of the velocity, it will vary approximately as the inverse square of the radius in a vortex of given intensity, and as the square of the intensity where the latter varies.

Where the resultant of all the forces hitherto mentioned tends to make a body approach the centre more rapidly than the contiguous filaments of fluid, the resistance to this motion will be greater the larger the under-water area of the body, projected in the direction of motion, and will also be greater the more the body departs from the spherical form.

Still a further action complicates the behaviour of a floating body. Owing to the friction of the lower layers of water over the bottom of the containing vessel, the velocity of whirl at a given radius is less than at points in the same vertical but nearer the surface. The centrifugal force of the bottom particles thus becomes insufficient to counterbalance the tendency to inward flow caused by the super-

* These deflections of the wake were shown by means of colour bands of aniline dye, escaping from small holes pierced around the periphery of a hollow cylindrical floating body, containing aniline solution.

elevation of the outer layers, and, in consequence, the velocity of inward flow increases somewhat from the surface downwards, and is a maximum at the bottom. This increases the relative tendency to inward motion of a body whose depth of immersion is relatively great.

From the foregoing analysis it appears that, depending on the relative magnitude of the forces called into play, one body may be irresistibly attracted to the centre of the vortex, a second may be actually repelled from the centre, a third may circle in neutral equilibrium at some definite radius, while a fourth, again, depending on the size, shape, and distribution of weight, may be attracted inwards to a certain radius of stable equilibrium at which there is an exact balance between the inward and outward forces, and within which the resultant force is outwards. Others again, and this is very definitely shown by the experiments, may have two radii of equilibrium, the space between being one of neutral equilibrium.

§ 4. *Experiments on Forced Vortex.* The forced vortex experiments were carried out in a circular vessel 15 inches diameter and 8 inches high. This was fitted to a whirling table and was capable of rotation at any speed up to 100 revolutions per minute by means of a small electric motor. The same series of floats were used as in the free vortex, and as a result of the experiments the following conclusions may be drawn.

(*a*) Small bodies approach the centre with a radial velocity which is greater the greater the radius of rotation and the intensity of the vortex.

(*b*) In homogeneous bodies of the same size and shape the heavier shows the less tendency to approach the centre.

(*c*) A non-homogeneous body of the same size, shape and weight as a homogeneous body, shows a lesser tendency to approach the centre. If the centre of gravity of the non-homogeneous body is sufficiently low, the body works out to the outer edge of the vortex.

(*d*) The shape of the plane of flotation (round, square or rectangular) has no effect on the behaviour of the object. This is, however, only true so long as the vortex is a true forced vortex, *i.e.*, has the velocity everywhere proportional to the radius. Any acceleration or retardation of the whirling table, and hence of the containing vessel, caused a sensible modification of several of the phenomena.

(*e*) In similar homogeneous bodies of the same specific gravity the smaller appear to show the greater tendency to approach the centre, although this effect is not strongly marked.

§ 5. The forces called into play in a forced vortex, while of the same general kind as in the free vortex, differ in relative magnitude, and, in some cases, in way of action owing to the fact that the velocity of whirl now increases with the radius, varying directly as the distance from the centre. A further difference follows from the fact that the fluid now moves in a series of concentric circles instead of in equiangular spirals.

Thus, due to the pressure of the surrounding vortex, apart from any consideration of the position of the centre of gravity of the body, the latter would tend to remain in equilibrium at any radius, with no tendency to approach or recede from the centre.

If the positions of the centre of gravity and of the centre of buoyancy do not coincide, centrifugal action, as in the free vortex, tends to produce radial motion, which is inward or outward according as the centre of gravity is above or below the centre of buoyancy, while the same general considerations as regards the effect of different specific gravities, vertical dimensions, and depths of immersion, hold true as in the free vortex. The magnitude of the effective centrifugal force is still given by

$$\frac{W}{g} v^2 \left\{ \frac{r - r_1}{r^2} \right\} \text{ lbs.}$$

but as v is proportional to r this force is independent of the radius of rotation except in so far as the inclination of the vertical axis of the body varies with the radius. In the forced vortex the inclination increases with the radius so that this force now diminishes as the body approaches the centre and *vice versa*.

As in the free vortex, the magnitude of the force at any radius varies as the square of the intensity of the vortex motion.

In the forced vortex, since the velocity increases as the radius there is no relative tangential velocity, and so no relative rotation of water and solid.

Where, however, any retardation of the containing vessel takes place, or where a vortex is produced by stirring in a stationary vessel, there is, due to the retardation of the outer filaments by friction, a tendency to inward radial flow along the bottom, accompanied by an outward radial flow over the surface layers. Also, since the tangential velocity does not now increase proportionately to the radius, there is a relative tangential motion of solid and fluid which results in a rotation of the

body, relative to the surrounding water, in the opposite direction to that of rotation. Each of these secondary phenomena gives rise to a tendency to outward motion of the object, which tendency is more marked as the depth of immersion, the size, and the departure from the circular form, of the object increase.

In conclusion, the Author would express his indebtedness to Prof. J. E. Petavel, F.R.S., by whose courtesy he was able to make use of the resources of the Engineering Laboratories of the Manchester University for a part of the experimental work.

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VIII. Studies in the Morphogenesis of certain Pelecypoda. (1) A Preliminary Note on Variation in *Unio pictorum, Unio tumidus* and *Anodonta cygnea.*

By Margaret C. Marcii, B.Sc.

(Communicated by Dr. G. Hickling.)

Read December 13th, 1910. Received for publication January 17th, 1911.

In studying the British fresh water Unionidæ, *Anodonta cygnea*, *Unio pictorum* and *Unio tumidus*, with a view to ascertaining the amount of variation in form in a well defined modern species, several facts of general application became clear.

Relation between variation in form and environment.

1. From the study of the form of *U. pictorum* and *U. tumidus* from the collections of Mr. Standen, the late Mr. R. D. Darbishire, Mr. J. W. Jackson, and the Conchological Society it became evident that there were two main types of shell, the first stout and heavy with relatively long dorso-ventral and lateral axes and short antero-posterior axis. (*Fig.* A of *Plate.*) The other form was the antithesis of this.

A further indication of the existence of such forms was obtained by variation curves, taking antero-posterior or lateral axis in relation to the dorso-ventral, Although the numbers were too few to give a definite proof (132 *U. pictorum*, 75 *U. tumidus*, 114 *A. cygnea*) yet they gave an indication of the presence of two such forms.

March 14th, 1911.

This curve had two maxima, one at ·68, the other at ·75, ·76, ·77 for *U. pictorum*. On looking up the geographical distribution of the two forms it was apparent that those with a maximum of ·76 and ·77 were found either on Keuper marls or in districts where, as at Wistow

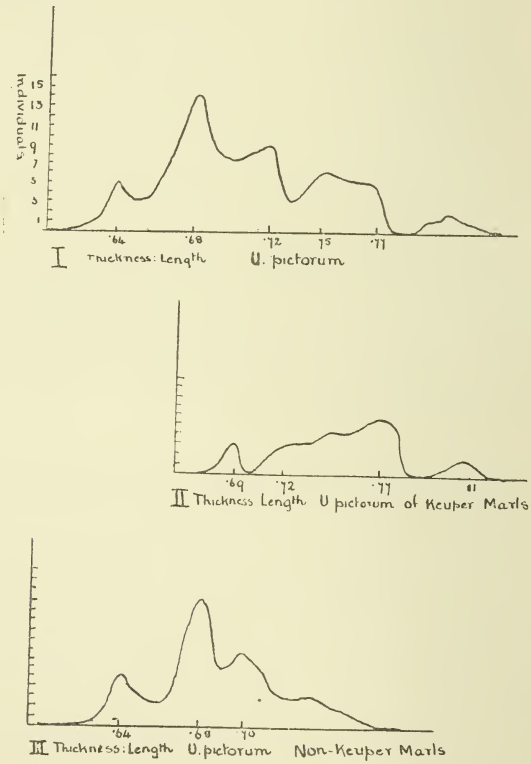

Fig. 1.

Thickness : Length curve of *U. pictorum*.

Park, near Leicester, the boulder clay was derived in part from the denudation of a Keuper marl district.

On separating out the Keuper from the non-Keuper forms, two simple curves were got, each with a single

maximum, corresponding to the two maxima of the first curve.

The non-Keuper curve, however, still contained a second sub-maximum at ·74. Investigation showed that this was largely due to forms from the New River and Thames at Twickenham, both districts in the London Clay. The latter specimens did not give such high ratios as those from the New River, possibly owing to the greater amount of chalk in the Thames.

A marked discrepancy in the Keuper marl curve was modified by the removal of forms from Congleton, where the river in which they were found, although actually passing through Keuper marl, yet flows for the greater part of its course through Carboniferous beds.

Forms from Evesham resembled those from the Keuper marl in outline, but not in breadth and thickness ratio. At Evesham the Avon is running through Lias, but the greater part of its course is through Keuper marl. Their removal smoothed out the curve.

The curves for *tumidus* gave a similar result with maxima at ·74 and ·66-·65. In the *A. cygnea* curve a similar result was hinted at, but the number of specimens from Keuper marl districts was too few to make a curve as definite as in Unios.

2. Another difference of form is visible amongst the thinner shelled non-Keuper specimens. The shells from the Marple, and Burnley and Accrington (*Figs.* B & C of *Plate*) canals are remarkable for their extreme length, and many of the Marple specimens for a forward throw of the umbo, giving a form very like the intertidal Modiolas. This alteration in shape occurs during the growth of the shell as is shown by the growth lines. Anteriorly the growth lines are close together, posteriorly they are far apart. This points to

the fact that anterior growth is slower than posterior, as is natural in an animal which has to plough its way through the mud. An increase in the rate of the current in which they lived would produce a decrease in pre-umbonal development, and so tend to throw the umbo forward in forms living in strong currents.

Usually rivers are far swifter than canals. But the canals in which these elongated forms occur have steep gradients, and therefore many locks. Strong lock currents are felt at any rate near the locks. The fact that these forms occur in canals with strong gradients, the more modified ones in the steeper and more numerously locked canals, seems to indicate a correlation between the two facts, although I cannot ascertain the exact locality,* with regard to locks, in which the specimens occur, nor the distance to which the lock currents are felt. Current action has been shown to be responsible for the curious "Platiform" varieties from the river Foss and Lake Rudyard. (*Figs.* D & E of *Plate.*)

Elongation is also produced by the quality of the mud in which they live, thick mud inducing elongation. But forms on a similar mud to the Burnley and Marple ones are not elongated as they are.

3. The relation between current and form accounts in part for the Keuper forms. The rivers of these districts are slow and so lead to the development of forms with a long dorso-ventral axis. It does not, however, account for the thickness of the shell, or the greater lateral axis. These must be due to the composition of the mud in which they are found.

It is a remarkable fact that the shells examined from districts highly charged with $Ca\,CO_3$ have thin shells and

* The majority of the Marple specimens were obtained from the lock basins themselves or close to the locks.

are not eroded at the beaks. This last is because the limestone districts are unfavourable for the production of bogs, and the banks of chalk and limestone streams are not covered so thickly with vegetation as those of streams in clay districts, so that the streams in chalk and limestone districts are not charged with humic acid and the shells in them are not eroded.

The greater thickness of the shell and animal in the Keuper districts seems to be connected either with the presence of humic acid, or the absence of excess of chalk.

4. *Anodonta cygnea* shows a remarkable change of form during growth. The once so-called species *A. anatina* has been definitely shown to be merely a growth stage of *A. cygnea.*

The glochidral shell (= phylembryonic stage of Aviculidæ.—R. T. Jackson) of this animal is exceedingly thin and has a perfectly straight hinge line when the valves are shut. The nepionic [umbonal] markings are less pronounced than in the two Uniones studied, and consist of more or less concentric ridges, varying in number and thickness.

From the commencement of the nepionic stage the young animal ploughs its way through the mud, with the result that growth at the anterior end is hindered, so the posterior end develops more rapidly. The unequal growth is more marked than in the typical Uniones, probably because the shell is far thinner throughout life in *Anodon.*

At the end of the third year the posterior end has so gained in growth on the anterior that the originally central position of the umbo has been lost, the post-umbonal region being longer than the pre-umbonal.

Anodons, when moving through the mud under the water, plough along with the head directed downwards, so

that the hinge line is inclined forwards in the natural position of the shell (*i.e.*, the hinge line, instead of being parallel to the tangent to the ventral surface, vertically below the umbo, would meet it at an acute angle if pro-

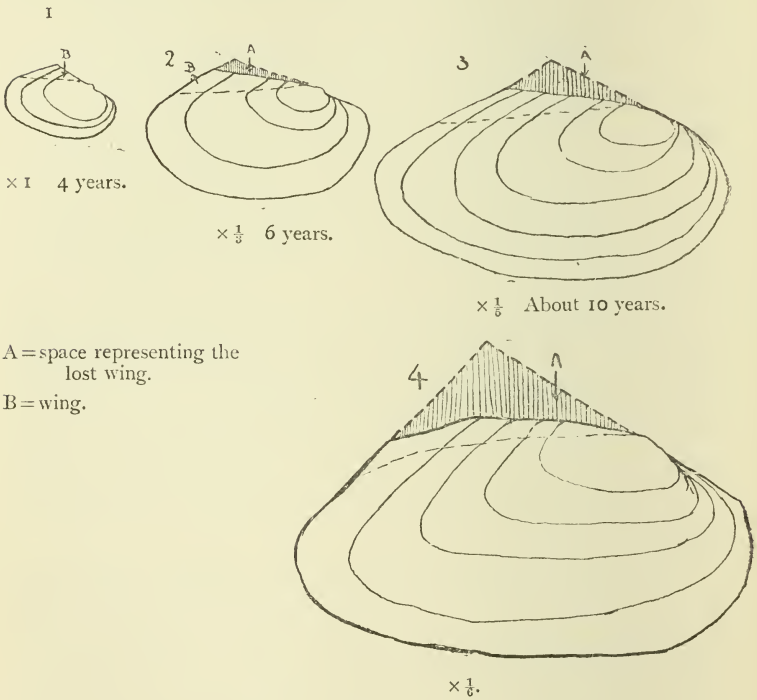

Fig. 2.

Diagrams illustrating the loss of the "wing" during growth stages of *A. cygnea.*

duced). The excess of growth posteriorly produces a flattened ventral margin, and removes all appearance of bilateral symmetry from the valves.

The body, however, keeps more or less parallel to the ventral tangent during movement. This is shown by the fact that the posterior ridge marking the most dorsal part

of the body is inclined downward and backward from the umbo when the shell is not inclined anteriorly.

This direction of the body leaves the posterior portion of the shell as a "wing" with closely opposed sides. The older individuals do not possess this wing,—the alation decreasing with growth. This disappearance has been said to be due to subsequent anterior development, but four facts point to wearing as the cause.

Firstly. The post-umbonal dorsal margin shows distinct marks of wearing, the edges being rough and broken.

Secondly. The ligament becomes more and more prominent with age. Though it does not, of course, occupy its original position, still its increasing prominence after the adult form has been reached, shows that wearing is still excessive in that part of the shell.

Thirdly. The pre- and post-umbonal dorsal edges are no longer in a straight line, but make a distinct angle with each other.

Fourthly. The post-umbonal lines of growth, if produced upwards, meet the pre-umbonal hinge-line produced backwards, and the enclosed space has the form and position of the "wing" of the younger stages.

This angle cannot have been formed by excessive anterior growth, because an angle thus formed would be re-entrant. Also the growth lines show posterior growth to be greater than anterior.

The proof that the original slope of the hinge line is due to tilt and not to growth is also shown by the fact that upward posterior growth would produce a re-entrant angle.

The disappearance of this part would be accelerated by two facts.

(*a*) Its position would be a hindrance to an animal creeping through mud or water, and therefore any tendency to lose it would be advantageous to the individual.

(*b*) It is formed during early growth of the shell, and is therefore thin, and not likely to be especially strengthened as it contains none of the body.

The extreme sensitiveness of the Unionid shell, as shown by the study of the British fresh water species, makes it quite impossible to classify these Lamellibranchs on form alone. If nothing but the shell were known, distinct species would be made out of the typical Ossington forms (*Fig.* F of *Plate*), the large, tumid, Birmingham types (*Fig.* A of *Plate*), the elongated Marple specimens with the forward throw of the umbo (*Fig.* B of *Plate*), the long Burnley shells with the normal umbo (*Fig.* C of *Plate*), and the platyrhyncoid Foss and Lake Rudyard forms (*Figs.* E & D of *Plate*),—leaving out of account the distinctions that could be drawn in these types, based on a study of umbonal markings.

Ornament in British Uniones.

The ornament in the British Uniones is confined entirely to the nepionic stage, and is therefore probably of phylogenetic importance.

The markings are of three types :—

(1) *U. pictorum type*, which consists of two divergent rows of V-shaped "tubercles," which may be joined.

(2) *U. tumidus type*, where the inner limbs of the "V's" are joined and shorter than the outer ones,

giving the appearance of a strongly up-looped ridge.

(3) *A. cygnea type*, here the ornamentation resembles *U. tumidus*, except in that the uploop is far more faintly marked, even ultimately disappearing.

Umbonal markings are taken as being of some importance in the separation of modern species. The genus *Pseudanodonta* has been founded largely on this.

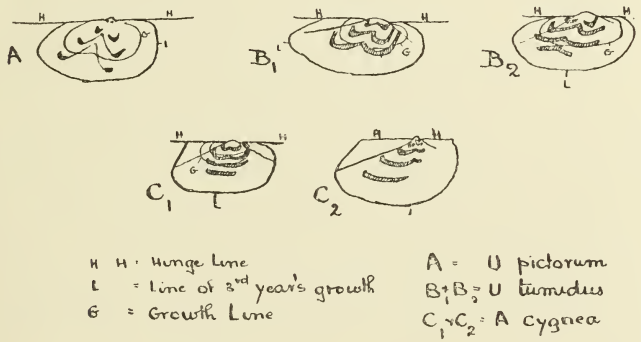

Fig. 3.

Diagrams showing typical umbonal markings in the British fresh-water Unionidæ.

The British species studied by no means keep to their type. Pictorums, otherwise typical, have been found with markings sometimes resembling and sometimes even identical with those of *U. tumidus*. This latter shows intermediate stages between its own type and *U. pictorum* on the one hand and *A. cygnea* on the other.

Young Anodons collected from the same pond or place on a river show two varieties of markings, with intermediate stages (*Fig.* G of *Plate*). Their ornament may consist of few, well-marked or numerous poorly

defined ridges, so that, at any rate in these species, umbonal markings are valueless for specific classification.

Relationship amongst the British fresh-water Uniones.

The study of these molluscs leads to the consideration of their relationships. Evidence concerning this can be obtained from two sources, viz., the early shell with its umbonal markings, and the adult shell.

1. The three types of umbonal marking in the British species have already been described. The meaning of these markings is shown in the development of young Anodons. The ornament in the youngest Anodons consists of two rows of "tubercles" joined into a **W**, later the limbs widen and form a **W** with a short wide inner part, later still, the inner limbs die away, leaving an even ridge. This points to the fact that the umbonal markings of *Anodon* pass through three phases, the first resembling *U. pictorum*, the second *U. tumidus*, the last being its own normal form.

A similar reading of the nepionic ornament of *Anodon* is given in the East African forms where the markings pass on to the neunic stages of the shell. Here they start as "tubercles," which become prominent, forming a pronounced **W**, which is more stronly marked where it is crossed by growth lines. The inner limbs of the **W** become less and less marked, and the outer limbs form a wider and wider angle with each other, until they lie practically in the same line ⌣.

2. In the adult shell the chief differences are:—

(1) The absence of teeth in *Anodon*.

(2) The presence of alation in *Anodon*.

(3) The absence of lunule in *Anodon*.

The secondary character of the edentulousness of *Anodon* is shown by two facts :—

(*a*) The recurrence of rudimentary teeth in young and adult Anodons.

(*b*) The occurrence of an almost perfectly graded series in the American forms, which, formerly included with the Unionidæ or Anodontidæ, are now mostly placed in an intermediate group, the Lampsilidæ. The first members of this series have perfectly developed true Unionid teeth, the last are truly edentulous.

This loss of teeth occurs along two lines.

(*a*) The series { *Hyriopsis bialatus.* / *Lampsilis laevissimus.* / „ *purpuratus.* / *Cristaria herculea.* / British and many American Anodons.

Here the cardinals pass from a flattened, modified form to entire absence,—the laterals becoming more gradually rudimentary.

(*b*) The series { *Lampsilis alata.* / „ *complanata.* / *U. pressus.* / *A. fragilis.*

Here the laterals disappear before the cardinals. Thus the edentulous forms have a double origin,—the British Anodons belonging to one series and the Magaritanas to a second.

Alation is unknown in the British Unionidæ, but is common in the American Lampsilidæ (formerly classed as Unionidæ). It is a remarkable fact that the development of alation is associated with the loss of teeth. These characters may be connected with habits or conditions

which cannot have occurred amongst the British Uniones and which must have been of a secondary character.

3. The lunule, which, absent in *Anodon*, and but feebly represented in some Uniones, is well developed in some American forms and even in early, probably Jurassic, Margaritanas.

In the study of the Unionidæ, Mr. J. W. Jackson has noticed that it disappears with the loss of the pseudo-cardinals. This absence, then, in *A. cygnea*, is due to the loss of these teeth, and with it the necessity for great width in the anterior part of the shell. This last difference, then, is of a secondary character.

Amongst the British Unionidæ, therefore, *U. tumidus* appears to retain the most primitive features both in nepionic and ephebic stages, and so to resemble the parent stock most closely. *Anodon* is the most highly specialized. *U. pictorum*, though occasionally showing a more strongly marked W than either of the other two, yet usually has the limbs of the W undeveloped, thus leaving unconnected "tubercles."

Pictorum and *Anodon* then, represent divergent cases of degeneration from *tumidus*. In the former case the limbs of the W have died away, in the latter the inner limbs have become so stretched out as to form a more or less concentric line.

The more primitive ephebic characters of *tumidus* are seen in its better developed teeth and marked anterior buttress.

The gaps between *Anodon* and *U. tumidus* can be filled in from the American fauna.

Ornament in Foreign Unionidæ.

The history of ornament in the Unionidæ since Triassic times has been one of degeneration from fully ornamented

types to smooth forms. Neumayr (Neumayr 1889), in his paper on *Trigonia*, says that all Unionid ornament is post-Miocene, and therefore atavistic. That can hardly be, since the Laramie Unionids are fully ornamented. He also states that the European forms are entirely unornamented. In this, he leaves out of account the umbona markings.

The ornament in foreign forms is of three types :—

(*a*) Forms in which it is confined to the nepionic stages (as in British forms).

(*b*) Forms in which it occurs on the adult shell, and is either phylo-ephebic or phylo-gerontic in character.

(*c*) Forms in which a phylo-gerontic smooth stage is followed by irregular ornament, which may therefore be regarded as phylo-hyostrophic in character.

(*a*) The umbonal markings show three, and probably, four types of ornament ;

(1) The **W**-shaped ridges, shown by many N. American, S. African, and European forms (possibly excepting the Margaritanas).

(2) Single **V**-shaped ridges, seen in some S. and N. American forms, *U. nyassensis*, and a form from Madras.

(3) Ridges which run diagonally up to the gonal line of the young shell.

(4) Possibly a type from the Plate River and Guiana. Here the ridges appear to be purely radial, but I am not certain whether they are purely radial or only a variation of the **V** type which can approximate a radial type fairly closely.

(*b*) The most definite phyloephebic ornament is seen in the forms from the rift lakes of E. Africa. *U. nyassensis* ornament begins as two diagonal lines joined by a concentric bar, but later alters into a **V**. Specimens from lake Miverw, are purely **W** marked throughout.

In *Quadrula lachrymosa.* (= *U. lachrymosus* [Lea]) the ornament is carried on from nepionic stages, dying away in old age, but is more or less distinct throughout.

In *U. ligamentinus* the ornament consists merely of strongly marked growth lines, and is clearly phylogerontic.

(*c*) *Q. Pustulosa.* (*U. pustulosus* [Lea]) starts life as a smooth shell, in extreme old age it developes irregular pustules, the ornament here is purely phylo-hyostrophic.

I hope to obtain further evidence on the nature of Unionid ornament.

The Phylogeny of the Unionidæ.

It is impossible to study any branch of the Unionidæ without considering their phylogeny. Two possible lines of descent are admitted,—the one through the Cardiniidae, and so in relation to the Carbonicolas. This view, according to Zittel's (Zittel, 1900) summing up, is the general one. The other, and older view, gives them a Trigonid ancestry. Several points seem in favour of this connection with the Trigonias.

(1) Dentition.

(2) Form of the shell.

The typical Trigonid dentition consists of short lateral teeth just in front of and behind the umbo, with the antero-laterals (pseudo-cardinals) supported by a strong buttress.

The typical Unionid teeth consist of much elongated posterior laterals and anteriorly placed antero-laterals, either without any buttress or with only a weak one.

The extreme length of the posterior laterals is probably connected with their greater post-umbonal development owing to their habitat in a region of continuous current. The change from typical Trigonid to typical Unionid teeth can be bridged over by American forms :—

(1) *U. ellipsis* has sub-equal ridged teeth with the anterior strongly buttressed.

(2) *Tetraplodon ambiguus* (= *Castalia* [Lam])[Pernambuco] has a somewhat elongated posterior lateral and an anterior buttress.

(3) *Quadrula trigona* (= *U. trigonus* [Lea]) has typical Unionid teeth, but with a strong buttress.

(4) *Pleurobema bigbyensis* (= *U. bigbyensis* [Lea]) has teeth as in *Quadrula*, but with a much reduced buttress.

2. The original description of Trigonias was based on their form, with two triangular areas, the smaller, the "area" being posterior. Modern adult British Unionidæ show no such distinction on their valves. It is seen, however, in *Q. lachrymosa*, and in the S. E. African forms. It is distinguishable on the umbones of the British and American forms. The ornament in the two groups is, I believe, similar, but this point requires further working out.

The great differences in structure, viz. :—

(1) the entire absence of a lunule in Trigonias ;
(2) the presence of an ant. buttress in „
(3) the absence of an accessory pedal scar in „

can be shown to be due to progressive variation, and can be bridged over by intermediate forms.

(1) The entire absence of a lunule in Trigonias seems to be associated with the position of the teeth.

As already shown the *disappearance* of the lunule is associated with the loss of anterior lateral teeth. Its *appearance* may be connected with the forward shifting of these teeth. In Trigonia they lie close under the umbo, where the shell is naturally wide, but after they have moved forward, they get into a region where the sides of the shell lie more closely together, especially if the animal becomes elongated, and where some modification is needed to give room for the teeth. This is done by the lunule. Also the lunule may or may *not* be present in Uniones.

(2) The link forms between non-buttressed and Unionidæ and strongly buttressed Trigonias have already been given.

(3) The absence of an accessory pedal scar in Trigonias is paralleled by its absence in Quadrula. Mr. J. W. Jackson has suggested that its absence is due to the shifting of the muscle from the buttress, where it was originally attached, on to the shell.

The similarity in the essential shell characters, and possibly in the ornament, together seem to point conclusively to close relation between the Unionidæ and the Trigonidæ. This relationship can only be that of a common ancestry from a pre-Triassic, pre-Trigonid stock, as the Unionidæ are known from Triassic times as well as the Trigonidæ. The very distinct ornamentation amongst the Liassic Trigonidæ, paralleled in the Triassic Myophorias and Trigonias, puts their convergence back into Permian times at least. I think it probable that the study of ornament in the Unionidæ will show the presence of as distinct, and possibly as ancient, lines as in their relatives.

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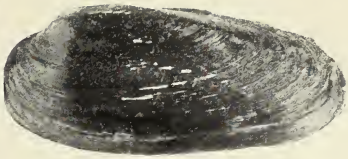

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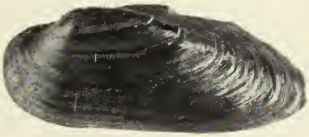

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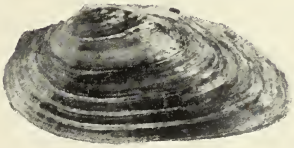

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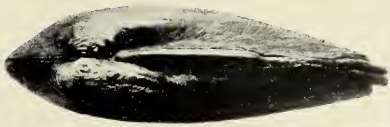

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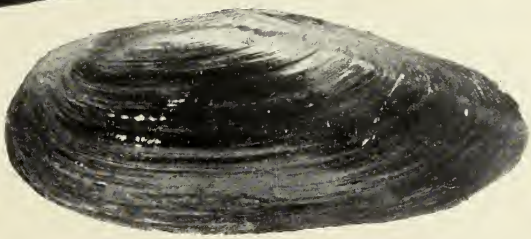

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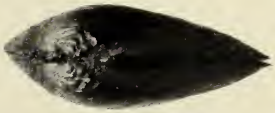
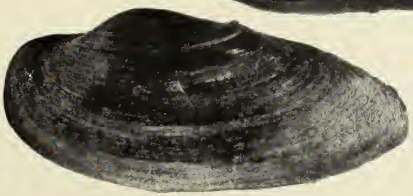




F

Photos by J. W. Jackson, F.G.S.

A. Typical Keuper *pictorum*.
B. Marple form, showing the forward throw of the umbo.
C. Burnley Canal form.
D. R. Foss form.
E. L. Rudyard form.
F. Typical non-Keuper form.
G. Young Anodons, showing umbonal markings [e. coll. R. Standen]
H. *U. pictorum* [Marple] showing tumidus like umbo.
I. Typical *U. pictorum* umbo.
J. ,, *U. tumidus* markings.
K. *U. pictorum*, } showing umbonal markings approaching *Anodon* in type.
L. *U. tumidus*, }

Manchester Memoirs, Vol. lv. (1911), *No.* 9.

IX. On an Abnormal Spike of *Ophioglossum vulgatum*.

By H. S. Holden, B.Sc., F.L.S.
(Lecturer in Botany in the University College, Nottingham).

(Communicated by Professor F. E. Weiss, D.Sc., F.L.S.)

Received and Read, January 10th, 1911.

The Ophioglossales, comprising the genera *Ophioglossum*, *Botrychium*, and *Helminthostachys*, have long been a subject of considerable interest to the botanist, a fact largely due to their extreme peculiarity and modification, and also to their comparative isolation from the remaining pteridophyte groups.

The question of the affinities of the group has always been a thorny one. Bower has considered their affinities to be with the Sphenophyllales: Tansley leaves the matter open but suggests a common ancestry with the remaining megaphyllous pteridophytes: Lady Isabel Browne considers them to show near relationships with the ferns proper, a view which receives considerable support from the most recent work, notably that of Chrysler, which brings forward the strongest evidence for this view. Their characteristic megaphylly, their stelar anatomy, and their undoubtedly fern-like antheridia and multiciliate sperms all lend support to filicinean relationships. The evidence afforded by *Botrychioxylon*, a fossil form described by Dr. Scott in his "Studies," is also of some interest. This is a Botryopteridean axis showing, apart from the internal wood, a striking resemblance to the modern genus *Botrychium* in stelar structure, and is considered by Scott to be distinctly in favour of the close relationships of the two groups.

March 21st, 1911.

Many of the features of the Ophioglossales are, however, essentially peculiar to themselves, perhaps the most marked being the division of the, usually, single leaf into a well-defined sterile and fertile lobe or segment.

Chrysler has recently revived the view, originally put forward by Roeper, that the fertile spike represents two fused, fertile, basal pinnæ of a pinnate leaf, and considers them derived by a further elaboration of the type of fertile pinna characterising *Aneimia*. The anatomical evidence upon which he bases his adherence to this view, appears, moreover, to be extremely convincing and is decidedly stronger than any points so far urged against it.

If Chrysler's view is to be regarded as correct, it would appear that the members of the genus *Ophioglossum* are derived through a simple-leaved ancestor, from the pinnate-leaved common ancestor of the group, and that they represent a reduction series culminating in *Ophioglossum simplex*. *Ophioglossum palmatum*, on this view, must be regarded as showing secondary specialisation after the assumption of undivided leaf structure.

The production of a branched fertile spike, as Bower points out, appears to be intimately connected with the lobing of the frond, and indeed from the figures given by that author ("Origin of a Land Flora," p. 440, figs. *c-g*). there is little in the external appearance, apart from the greater projection of the sporangia, to distinguish his *Botrychium simplex forma simplicissima*, from a small specimen of *Ophioglossum*.

This projection of the sporangia in *Botrychium* does not appear to be a feature of primary importance, since, according to Goebel, it has arisen owing to the pressure exerted upon the sporangial cells by the cells of the axis lying immediately beneath them.

The fertile spike, in normal specimens of *Ophioglossum*, consists of a fairly long, unbranched axis, oval in section, and bearing apically a double row of large, more or less globular sporangia.

The vascular supply is apparently subject to considerable variation (Cf. *Text-fig.* III. *a*, *b*, *c*) but in the sporangium-bearing region it appears constantly to consist of a central, purely cauline strand, and two lateral strands, the latter of which give off traces between each adjacent pair of sporangia. The lateral and median strands are connected by obliquely transverse strands at fairly frequent intervals. (*Text-fig.* I.).

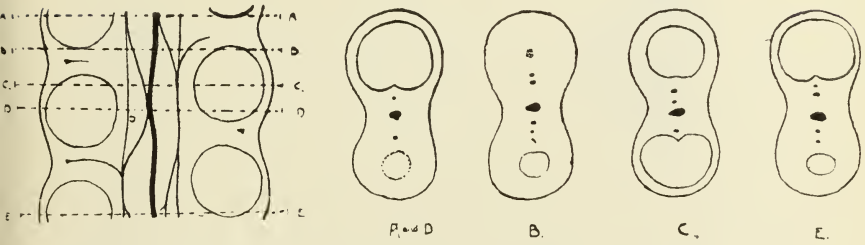
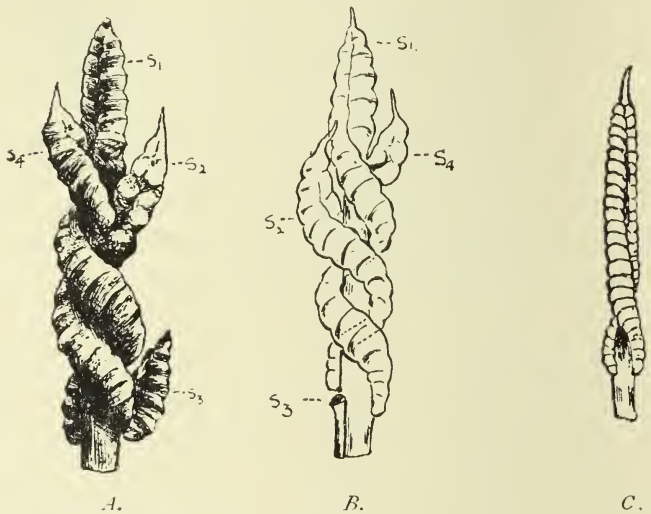


Fig. I. The diagram to the left represents a camera-lucida drawing of the outlines of part of the fertile region of a sporangiophore with its vascular supply. *Figs.* A to E represent the variation in the vascular supply as seen in transverse sections at the points indicated.

Ophioglossum, in common with other members of the group, is also characterised by the relatively frequent occurrence of abnormal forms, and these monstrosities have received considerable attention both from Bower and Chrysler, and others. (Cf. Bower, :08, p. 438, *fig.* 239, *j* & *k*). The abnormal specimen, which is the subject of the present paper, was collected some years ago by Professor Carr, from a locality near Skegby, Nottinghamshire, and he very kindly handed it over to me for description. The peduncle was somewhat stouter than the

average, but apart from this there was nothing abnormal in its appearance: the fertile region, however, exhibited very considerable abnormality and complexity (*Text-fig.* II. *A*, *B*).



A. *B.* *C.*

Fig. II.

A. Enlarged drawing of an abnormal spike S_1, S_2 Main Series, S_3, S_4 Accessory Series.
B. Semi-diagrammatic drawing from opposite point of view to *A*.
C. Slightly abnormal specimen showing rotation of sporangia.

The main structure consisted of two series of sporangia, coiled spirally round the axis, these separating near the apex of the axis, and developing in that region the double row of sporangia characterising the normal Ophioglossum "spike" (*Text-fig.* II. *A*, *B*; s_1, s_2).

In addition to these the sporangiophore bore basally and laterally, a small and apparently independent fertile portion, exhibiting the normal structure, and a similar but less completely independent one, near the point at which the two main series diverged. (*Text-fig.* II. *A*, *B*; s_3, s_4).

Judging from the external appearance of the spike there appear to be two possible interpretations of its structure. The first is that it represents two, or possibly more, fused spikes, when it would be a further elaboration of the condition figured by Chrysler for *Botrychium* (*Ann. Bot.*, 1910. *Pl.* II., *figs.* 26-28), in which the second, as well as the first pair of pinnæ have become fertile and partial fusion has occurred. In the case of the specimen recorded here, if this interpretation were correct, the fusion is much more complete than any of the abnormalities cited by Chrysler. The alternative view is that the normally unbranched spike has undergone a process of division or chorisis, a feature which normally obtains in *Ophioglossum palmatum*. It may be as well, perhaps, to state here that the anatomical evidence is in complete accord with this latter interpretation. The spiral arrangement is a further complication, but this feature is foreshadowed, to a certain extent, in specimens such as that shown in *Text-fig.* II. *C*. These slightly abnormal specimens are apparently of fairly frequent occurrence, no less than eight occurring in the class material at the University College, this being obtained from Aberystwyth and York respectively.

It was decided to cut serial sections of the sporangium-bearing region and of the peduncle, but owing to the imperfect preservation, and the quantity of air in the tissues, which even prolonged treatment under the air-pump failed to dislodge, this proved a task of considerable difficulty, certain portions shattering very badly. The material yielded, however, a sufficiently good series to elucidate the more important anatomical features. The peduncle was first examined and, as might have been expected, showed a comparatively massive and complex vascular system (*Text-fig.* III. *D*).

For purposes of comparison hand sections of twenty normal peduncles were cut, at approximately equal intervals, and were found to fall into three series.

The first of these, which I propose to term the "robust" type, exhibited five strands at the end nearest its union with the sterile segment, these dividing up to form a maximum number of eight bundles, anastomosing with their fellows at various points in their course through the peduncle, and ultimately uniting to form a series of five in the region of the first sporangium (*Text-fig.* III. *A*, 1*a*—1*f*).

There was only a single specimen of this type in the number examined.

The second type, comprising sixteen of the twenty, and which I propose to term the "normal" type, showed roughly the same features as the first, commencing basally with five bundles, but exhibiting divisions and anastomoses which were less, both in number and frequency, than those of the first type. In the region of the first sporangium this type also exhibited five bundles in transverse section (*Text-fig.* III. *B*, 2*a*—2*f*).

The third type, under which the remaining three peduncles are included, is smaller than the others, and may be termed the "slender" type. In it the basal number of bundles was three. There were no signs of anastomoses, and the lateral bundles each divided once giving rise to five, at the commencement of the fertile region, as in the remainder (*Text-fig.* III. *C*, 3*a*—3*d*).

An examination of the appended text-figure (*Text-fig.* III., *A*, *B*, *C*) will serve to demonstrate then, that, though there is considerable variability in the number of vascular strands in the lower portion of the peduncle, the number entering the fertile region is very generally five.

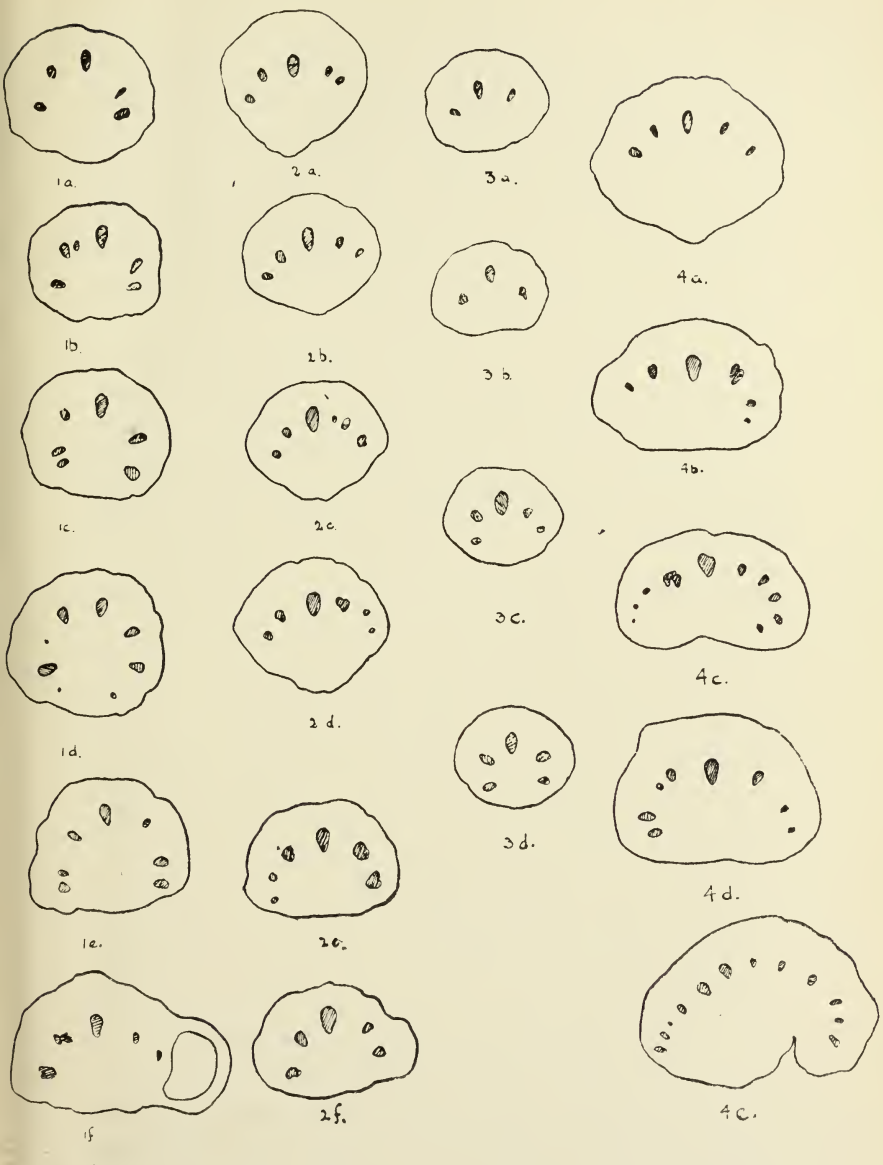


Fig. III.

A. Series of sections through a peduncle of the "robust" type.
B. ,, ,, ,, "normal" ,,
C. ,, ,, ,, "slender" ,,
D. ,, ,, the peduncle of the abnormal specimen.
(All the diagram outlines and the relative sizes of the bundles sketched with the aid of a camera lucida.)

Two other points are also worthy of mention, the first being that there is, throughout both sterile and fertile parts of the spike, a dominant median strand, and the second, that, a little way below the point of union with the sterile assimilatory portion, the vascular supply constantly consists of three strands. The results obtained are thus in complete agreement with the work of Campbell, and of Holle, upon the subject.

The peduncle of the abnormal specimen was in exact agreement with the normal forms as regards the possession of three vascular strands before separation from the sterile segment, and in having five strands immediately above the point of separation (*Text-fig.* IV. *Text-fig.* III. *D*, 4*a*).

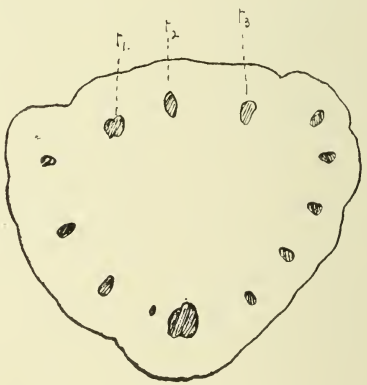


Fig. IV.

Tr. S. petiole just previous to the separation of the three traces (t_1, t_2, t_3) to supply the peduncle.

A very short distance above the base, however, the strands subdivide rapidly, until the number is considerably in excess of even the most robust of the typical specimens. Apart from these features, there is nothing abnormal in the behaviour of the vascular supply until it reaches

the region in which the small basal spike is given off. Here the bundles on the side nearer the spike divide up further until there are fourteen bundles shown in transverse section (*Text-fig.* V., 1). Those destined to supply the lateral spike now bend round and are nipped off gradually from the main system to form an accessory one (*Text-fig.* V., 2, 3). The small spike was cut off just above its base, and found, on sectioning, to exhibit a structure identical with that of a typical specimen.

A comparison of Bower's figures illustrating the supply to the lowest fertile spike in *Ophioglossum palmatum*, and the first three diagrams in *Text-fig.* V. will illustrate the striking parallelism exhibited by the two cases. Sections further up the stem revealed the fact that the subsequently produced structures (*Text-fig.* II. *B*, s_2, s_4) also conformed in a perfectly clear manner to this type of branching, differing only in detail, whilst the portion s_1 was found to be equivalent to the upper portion of the normal Ophioglossum spike, representing the continuation of the main axis. It therefore appears that the specimen described represents a single fertile spike, which, by a process of chorisis, has given rise to the branched condition typically found in *O. palmatum.*

A further point in support of this view is afforded by the perfectly normal character of the vascular supply to the parallel series of sporangia. Had these been the result of the fusion of two independent axes, either the vascular strands of these axes must have rotated correlatively with the sporangia to produce their vascular supply, or, in the event of there being no rotation, those sporangia on the side remote from the parent axis would have been devoid of vascular supply.

A careful examination of the whole series has convinced me that there is a perfectly normal vascular supply,

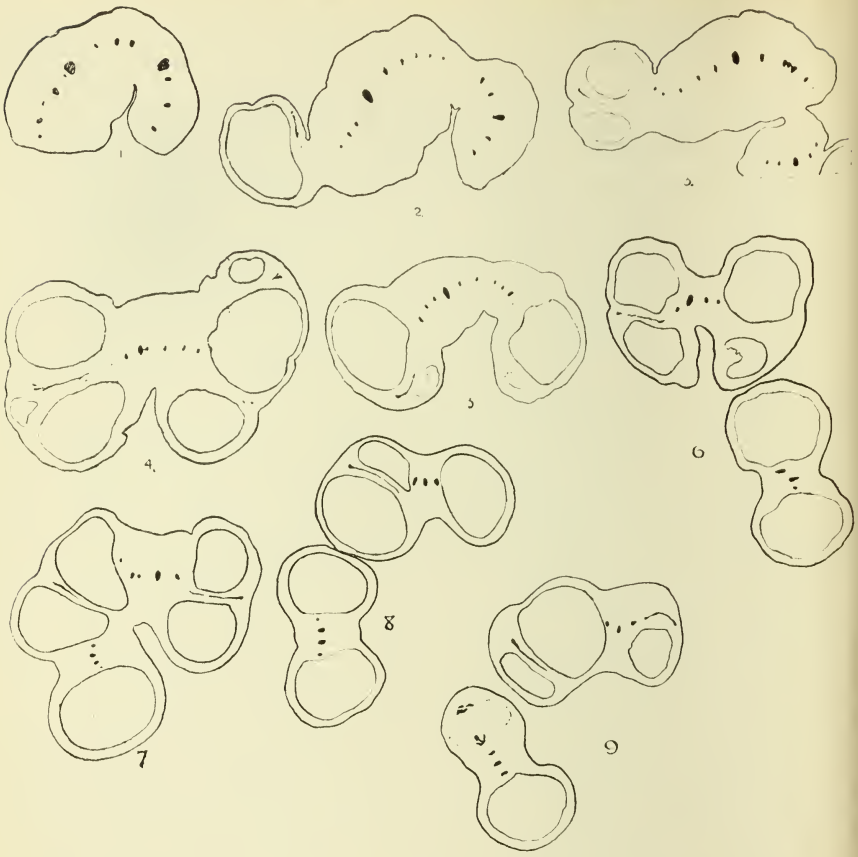


Fig. V.

Nos. 1-3 show the gradual separation of the vascular supply to the basal sporangiophore branch.

No. 4 shows the bundle supply to the sporangia on both sides of the main system.

Nos. 5-9 show stages in the formation and separation of the remaining branches.

(All figures drawn with the aid of a camera lucida.)

Fig. V.A. Showing the detail of the bundle on the left side of *Text-fig.* V. 5.

that is, that there is one strand passing between each pair of sporangia throughout (*Text-fig.* V. 4).

The spiral arrangement may therefore, quite reasonably, be regarded as a secondary modification, admitting of the insertion of an unusually large number of sporangia, and probably correlated with the crowded arrangement of the parts.

One last point in support of this view is that, though the number of vascular bundles throughout is considerably in excess of the normal, this is to be regarded as a necessary concomitant of the extra demands placed upon it, the characteristic, dominant, central strand being readily distinguishable from base to apex and terminating the main fertile spike.

Summary.

1. The specimen represents a condition of the fertile Ophioglossaceous spike derived by a process of splitting or chorisis.

2. Its robust character makes great demands upon the vascular supply and this is increased proportionately.

3. The view that the specimen has arisen by chorisis and does not represent two fused axes is supported by the following anatomical features :—

    i. There is a single dominant median strand throughout.

    ii. The sporangial vascular supply is normal.

    iii. The vascular supply to the accessory spikes is in close agreement with that figured by Bower for *Ophioglossum palmatum* and

supports his view as to the morphology of the fertile spike in this form.

iv. The vascular supply entering the peduncle from the petiole consists of three strands, of which the two lateral ones divide once, thus giving rise to five strands, as in the majority of normal spikes.

In conclusion, I should like to express my sincere thanks to Professor Weiss, for his kindly help, to Mr. S. Garside, B.Sc., of the Victoria University, who has examined the specimens of *Ophioglossum* in the herbarium of the Manchester Museum, on my behalf, and to Professor Carr, of the University College, who gave me the specimen, and in whose department the work has been done.

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Manchester Memoirs, Vol. lv. (1911), *No.* **10**.

X. The Boric Acids.

By Alfred Holt, M.A., D.Sc.

Received and Read February 21st, 1911.

An examination of the results of the researches carried out on the anhydrous borates of the alkalies and alkaline earths gives evidence of a number of compounds derived apparently from some more or less hypothetical boric acids in addition to those which are usually accepted as having a definite existence, *i.e.*, the ortho, meta, and pyro varieties.

Ditte (*C. R.*, 1873, 77, 785, 893) described compounds of the types $2MO.3B_2O_3$ and $3MO.2B_2O_3$, where M represents a metal of the alkaline earths, but Guertler (*Zeit. Anorg. Chem.*, 1904, 40, p. 337) has since shown that these represent eutectic mixtures. This latter author has found evidence for the following compounds : (1) $3MO.B_2O_3$, when M is magnesium or barium. (2) $2MO.B_2O_3$, when M is any of the alkaline earths. (3) $MO.B_2O_3$ and $MO.2B_2O_3$, when M is calcium, strontium or barium. The present writer has found that in the case of the sodium borates, compounds of the composition $Na_2O.B_2O_3$ and also probably $Na_2O.4B_2O_3$ exist, while the anhydrous stable crystalline form of borax is an eutectic mixture (*Proc. Roy. Soc.*, 1902, 74, 285).

April 20th, 1911.

Very little attention seems to have been paid during recent years to the boric acids, and most of our knowledge of these compounds is due to the researches of Ebelmen and Bouquet (*Ann. Chim. Phys.*, 1846, 3, 17, 63), Schaffgotsch (*Pogg. Annal.*, 1859, 107, 430), and Merz (*J. Prakt. Chem.*, 1866, 99, 179). Ebelmen and Bouquet, by examining the rate of dehydration of orthoboric acid, concluded that at least two other acids existed, and in support of this view described organic derivatives analogous to borax, and an orthoborate.

Schaffgotsch observed that when orthoboric acid is heated on a water-bath for some time, two-thirds of the water it contains is removed, leaving a vitreous substance which could only be dehydrated completely by heating to a very high temperature.

Merz confirmed the observations of Schaffgotsch, but considered that several acids containing less than a third of the water of orthoboric acid existed. He also noticed that orthoboric acid was volatile to some extent in the water vapour given off on heating it.

The molecular weights of neither the anhydrous borates nor meta and pyro boric acids are known, and consequently their formulæ are only empirical, but the constitution of the ortho acid is well established since it forms a volatile triethyl derivative.

The experiments described in this paper were carried out to see whether the meta and pyro acids were really definite compounds or mixtures, and whether any other acids existed.

In the first series of experiments a weighed amount of orthoboric acid was heated in a platinum dish at a constant temperature and the loss in weight determined from time to time.

(1) Temperature, 98°C.

Weight of acid taken = 3·89 grms.
After heating for 24 hours, weight = 3·35 „
„ „ „ 46 „ „ = 3·07 „
„ „ „ 95 „ „ = 2·74 „
„ „ „ 118 „ „ = 2·74 „
„ „ „ 125 „ „ = 2·74 „

3·89 grms. of orthoboric acid yield 2·76 grms. of metaboric acid. The loss of acid through volatilisation is very small.

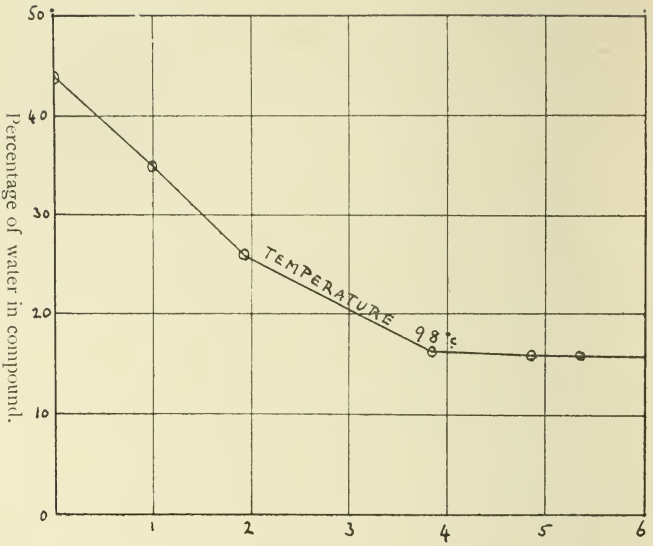
(2) Temperature, 120°C.

Weight of acid taken ... ... = 4·22 grms.
After heating for 1 hour, weight = 4·11 „
„ „ „ 3 hours „ = 4·04 „
„ „ „ 6 „ „ = 3·87 „
„ „ „ 10 „ „ = 3·71 „
„ „ „ 14 „ „ = 3·49 „
„ „ „ 19 „ „ = 3·23 „
„ „ „ 25 „ „ = 3·01 „
„ „ „ 30 „ „ = 2·95 „
„ „ „ 42 „ „ = 2·94 „
„ „ „ 52 „ „ = 2·94 „

4·22 grms. of orthoboric acid yield 3·00 grms. of meta acid. The amount volatilised in the water vapour is somewhat greater in this than in the previous experiment. This is probably due to the temperature being higher and the rate of dehydration being consequently faster.

(3) Temperature, 150°C.

Weight of acid taken ... ... = 4·14 grms.
After heating for 1 hour, weight = 2·78 „
„ „ „ 5 „ „ = 2·60 „
„ „ „ 8 „ „ = 2·54 „
„ „ „ 13 „ „ = 2·47 „
„ „ „ 29 „ „ = 2·32 „
„ „ „ 37 „ „ = 2·20 „


50
40
30
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Percentage of water in compound.
TEMPERATURE 98°c.
1
2
3
4
5
6
Time in Days.

Fig. 1.

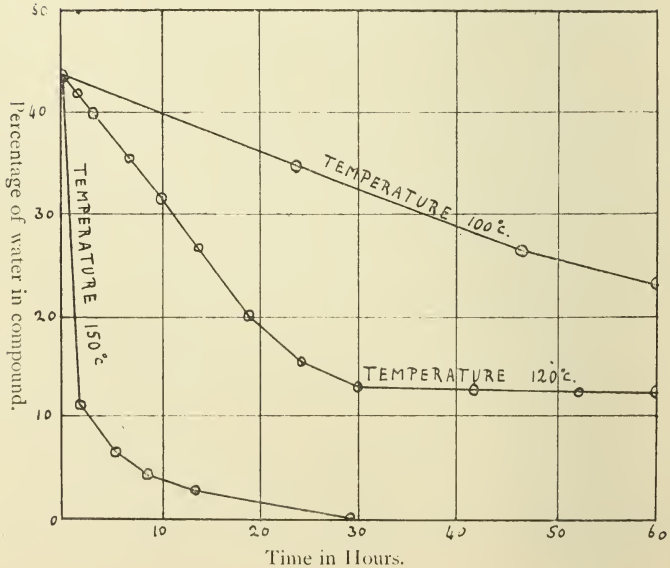
50
40
30
20
10
0
Percentage of water in compound.
TEMPERATURE 100°c.
TEMPERATURE 150°c.
TEMPERATURE 120°c.
10
20
30
40
50
60
Time in Hours.

Fig. 2.

4·14 grms. of orthoboric acid yield 2·92 grms. of meta acid and 2·64 grms. of pyro acid. In this case the loss through volatilisation is much greater, the final product weighing less than the theoretical amount for boric anhydride. It would, therefore, seem that the amount volatilised depends upon the rate of heating. These three experiments, only calculated to show the percentage of water present in the compound from time to time, are shown graphically in *Figs.* 1 and 2, from which it will be seen that only one break exists in the dehydration curve. It will be noticed in *Figs.* 1 and 2 that the break in the curve occurs with a lower percentage of water as the temperature at which the experiment was conducted was raised. This is due to volatilisation of the ortho acid in the water vapour. The difference between the percentage of water present in the compound when the break takes place and the theoretical value for metaboric acid (20·5 per cent.) gives a rough measure of the volatility of the ortho variety.

These results confirm those of other experimenters as to the marked change in the rate at which the ortho acid loses water, and probably show the definite existence of metaboric acid, but there is no indication of any other compound.

The second series of experiments consisted in the determination of the melting points of mixtures of orthoboric acid and boric anhydride. Intimate mixtures of the required composition of the two substances in a fine powder were made, and then introduced into stout capillary glass tubes, which, as soon as they were filled were sealed as close as possible to the top of the mixture. The melting point was then determined in a bath of sulphuric acid. It was necessary to use sealed tubes as

the orthoboric acid would otherwise lose its water on heating.

The following results were obtained :

Orthoboric acid (H_3BO_3)	melting point		169-170°.
$2H_3BO_3 . B_2O_3$	,,	,,	158-159°.
$H_3BO_3 . B_2O_3$	,,	,,	159-160°.
$2H_3BO_3 . 3B_2O_3$	,,	,,	167-174°.
$H_3BO_3 . 2B_2O_3$	,,	,,	172-173°.
$2H_3BO_3 . 5B_2O_3$	,,	,,	171-173°.
$H_3BO_3 . 3B_2O_3$	,,	,,	172-174°.
$H_3BO_3 . 4B_2O_3$	,,	,,	171-172°.
$H_3BO_3 . 5B_2O_3$	,,	,,	170-172°.

These results do not point to any definite compounds. When boric anhydride is added to orthoboric acid, the melting point of the latter compound is lowered. It subsequently rises again and then remains approximately constant.

In all mixtures containing more boric anhydride than is represented by the composition $H_3BO_3 . B_2O_3$, a small portion of the mixture melted at 158°-159°, the melting points given above being those of the main mass of the substance. It is possible that this is the result of imperfect mixing of the constituents in the capillary tubes employed, but more probably is due to the formation of two partially miscible solutions. Mixtures richer in boric anhydride than is represented by the composition $H_3BO_3 . 4B_2O_3$, when melted remained on cooling in the vitreous metastable state, for when heated to 110°-115° they gradually but completely crystallised. Boric anhydride itself could not, however, be obtained in the crystalline condition. In this connection it is interesting to note that metaboric acid apparently exists in both vitreous and crystalline states. When orthoboric is heated in air for some time at 100° an almost completely vitreous mass of the composition repre-

sented by metaboric acid results. If this heating has, however, taken place in a vacuum, the water evolved being absorbed by a drying reagent, the metaboric acid is a dry amorphous powder. Further, when a mixture of boric anhydride and orthoboric acid in equimolecular proportions is fused under pressure, a crystalline mass results.

The changes in vapour pressure on heating orthoboric acid were next examined.

The first experiments were carried out at 70°C. The vapour tension of the acid at this temperature remained almost perfectly constant for many days, when it suddenly dropped to about one-fifth its original value. When this drop in pressure occurred, a portion of the material was withdrawn and analysed, when it was found to contain 23·4% water. (Theory for metaboric acid 20·5 %.)

The rest of the material was then heated to 180°C. and the further changes in vapour pressure observed. It was necessary to employ this high temperature as the vapour pressure of the meta acid would otherwise have been too small to observe.

The observations at 180°C. were inconclusive as regards th formation of other compounds. The vapour pressure steadily decreased with time, and, though this decrease was not at a constant rate, no definite and marked changes occurred.

Some experiments were carried out by the freezing point method on the molecular conditions of the boric acids in aqueous solution. Three portions of the ortho acid were heated till one had the composition of metaboric acid, the second pyroboric acid and the third was completely dehydrated. Solutions of these portions, as well as the ortho acid, were then prepared of such a strength

that each might be considered to contain the same amount of boric anhydride per c.c. of water. The freezing points of these solutions were the same and identical with that of orthoboric acid, this condition remaining unchanged whether the solutions were very dilute or almost saturated.

Solutions of orthoboric acid from 2·48 grs. in 100 c.c. water to 0·1 gr. in 100 c.c. water gave depressions of the freezing point in accordance with a molecular weight of from 68·5 to 55 (theory for orthoboric acid 62). The acid was apparently very little ionised.

No solvent could be found for either boric anhydride or the other boric acids, in which they did not undergo change, so their molecular weights are unknown, but from the fact that they are vitreous substances in some ways analogous to the phosphoric and silicic acids, their molecular weights are probably greater than those represented by simple molecules.

Orthoboric acid is readily soluble in hot glacial acetic acid, from which it separates out unchanged on cooling. Boric anhydride does not appear to be soluble in this acid. It was hoped, by means of this difference in behaviour towards glacial acetic acid to decide whether the meta and pyro acids were mixtures of the ortho acid and boric anhydride or not.

Metaboric acid dissolves to a very slight extent in hot acetic acid the solution depositing the ortho acid on cooling, but the amount dissolved is far too small to correspond with that required for the meta acid if this compound were an equi-molecular mixture of the ortho variety with boric anhydride. The pyro acid does not seem to dissolve at all.

From the experiments which have been described, the following conclusions may be drawn :—

(1) Metaboric acid is probably a definite compound, or hydrate of boric anhydride.

(2) No clear evidence can be found for the existence of any acid containing less water than metaboric acid.

(3) Only orthoboric acid can exist in solution, under which conditions it is present in simple molecules.

(4) Metaboric acid cannot be regarded as an equimolecular mixture of orthoboric acid and boric anhydride.

(5) Fused mixtures of orthoboric acid and boric anhydride, in which the molecular ratio of the latter to the former compound exceeds 4·1, can exist in a vitreous metastable and a crystalline stable form.

Boron and aluminium being members of the same group of elements, analogies may be expected between them, and, as in the case of the latter element, a very complicated series of hydrated oxides are believed to exist, the same may be true for boron. This view would help to explain many of the experiments described in this paper, the ortho variety being the only definite acid of boron.

The University, Manchester.

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XI. Studies in the Morphogenesis of certain Pelecypoda. (2) The Ancestry of *Trigonia gibbosa.*

By Margaret Colley March, B.Sc.

(Communicated by Dr. George Hickling.)

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The Trigoniæ were subdivided by Lycett into eight groups. This classification was made in 1875, and was, therefore, based on adult shell characters. The subsequent work of Hyatt, Beecher, and Jackson has shown that the characters of true taxonomic value are nepionic. In trying to construct a classification of the Trigoniæ, therefore, the characters to note are those of the early shell.

The classification of other fossil Pelecypoda has been worked out along two lines, either on form or shell structure.

Alteration in form was used by Jackson in his classification of the Aviculidæ. In this case the group showed progressive adaptation to the sedentary habit, so the modification in form was biological. The Trigoniæ, however, show no such progressive adaptation,—their modification, therefore, is of a purely individual character and dependent on environment. Such purely œcological variation is of no taxonomic value.

Alteration in teeth structure, muscle scars, and pallial line has been used as the basis for a general classification. Variation in teeth can be used in the classification of the Unionidæ, which show perfectly graded series from

strongly toothed to totally edentulous forms. So far as has yet been worked out the Trigoniæ show a marked uniformity in teeth, muscle scars, and pallial line, the most marked variation being the development of ligamental pits in the Scabræ (Lycett). Some other method of classification must, therefore, be used in their case. This is obviously the development of their ornament.

Development of Concentric Trigonid Ornament.

The Trigoniæ are definitely known from the Trias, so that it might be inferred that in Triassic times the ornament would not have diverged from the simplest (*i.e.*, concentric) type. But this is not the case. Three types of ornament are known in Triassic times.

(*a*) Pure concentric. *Myophoria curvirostris.*

(*b*) Concentric, showing the introduction of tuberculate radial on the posterior part of the area. *M. lineata.* (*Figs.* I. and II. of *Plate.*)

(*c*) Tuberculate radial. *T. harpa.*

Since no intermediate types are known, it seems probable that the Trigoniæ are either polygenetic, or that their origin is so far pre-Triassic as to allow of the development of two of the three main types, and extinction of the intermediate forms of ornament by Triassic times.

However that may be, it should be possible to trace out the subsequent development of these types of ornament, and the first to be followed is naturally the concentric.

In the *Middle Lias* occurs *T. lingonensis.* This is purely concentric in ornament on both area and flank. Its later stages, however, tend to become smooth. Its importance, therefore, does not lie in its being in the main

line of descent, but in proving the continuance of this type of ornament in Liassic times.

In the *Inferior Oolite* occur two types with concentrically marked umbones, showing them to be derived from the pure concentric *M. curvirostris* type.

(*a*) *T. v-costata.* In these the area is pure concentric, as is the flank in nepionic stages. In later stages, however, these concentric flank markings break up near the marginal carina into alternating tubercles which rejoin diagonally, forming a marked V in that region of the shell. For the formation of at least one V one pair of anterior costæ unite to join one posterior limb. This is important, as it is a constant character throughout all forms which have V's or V-like undulations, and it is retained after the disappearance of this undulation.

(*b*) *T. costatula.* Here the ornament is purely concentric until late ephebic and gerontic stages where the flank markings begin to break up near the marginal carina into alternating tubercles.

Of these two types, *T. v-costata* seems to have reached a definitely phylephebic stage of ornament, and *T. costatula* to be still in a phylo-nepionic state.

The *Great Oolite* shows the continuation of *T. costatula* in *T. painii.* Here the nepionic stages are *M. curvirostris* and *T. costatula.* The ephebic show the introduction of further alternate pustulation, and the elongation of these pustules to form marked undulations near the marginal carina, though not the formation of a V as in *T. v-costata.*

Here also occurs *T. clytia* which is a descendant of *T. v-costata*, differing from it in the regular junction of two anterior costæ to one V after nepionic stages.

In the *Forest Marble* is found *T. undulata* which appears to be a further evolution of *T. painii.* In the

Cornbrash is *T. detrita* which is a later variety of *T. v-costata* and *T. undulata* again, which also appears in the *Corallian* with *T. geographica*, a closely allied form to *T. costatula.*

So far as I know no Trigoniæ of this type occur either in the Oxfordian or Kimmeridgian clays. But, as these deposits were laid down under very similar physical conditions, and as these forms occur before, between and after them, it appears quite legitimate to assume that the conditions under which these deposits were formed were unsuited to these particular Trigoniæ.

In the Portlandian times these forms with concentrically marked umbones re-appear and show a rapid development of ornament.

The simplest type is *T. Damoniana.* Here it is necessary to point out that two very distinct types are described under the name "damoniana" in Lycett's monograph from types both in Jermyn Street and in South Kensington. In both the area is concentrically marked at the umbo. These forms tend to lose the ornament on their areas in ephebic stages. The flank ornament passes through the curvirostris and costatula types during nepionic stages. When adult the markings consist of concentric costations which reach about half way across the valve, and then break up into the usual alternate pustulations, with, at least in one place, one pair of anterior costæ to one posterior row of tubercles. This marks the former existence of the undulation, though it is no longer visible. Its loss may, I think, be attributed to two causes.

(*a*) The greater isolation of the tubercles, which thus tend to lose their diagonal connection.

(*b*) The loss of ornament near the marginal carina, which leaves a clear space where the undulation should be.

The pustules in one type (*T. Damoniana* α) are small, In *T. Damoniana* β they are large.

The typical *T. gibbosa* of Lycett has reached a slightly further stage in the development of ornament than *T. Damoniana* α. Its carinal space is larger and better defined, and its costations are further broken up. Some Gibbosæ of Lycett exhibit total loss of costations, and some total loss of ornament. These may be counted as *T. gibbosa* β and γ, or possibly as gerontic stages of *T. gibbosa* α. The pustulations of *T. gibbosa* are larger than those of *T. Damoniana* α.

The ontogeny of *T. gibbosa* is excellently shown by young and adult specimens from a block of Portland stone in the Manchester Museum. Here the youngest shows a pure *M. curvirostris* type, the next a *T. costatula* stage and a later stage giving the introduction of an undulate type. These stages are again reproduced in well-preserved adults from the same block. (*Figs.* IV. to VIII. of *Plate.*)

T. Damoniana β, as figured by Lycett in his monograph, appears to correspond with *T. gibbosa* β, that is to say, its ephebic ornament is purely tubercular, the *T. gibbosa* α and *T. Damoniana* α types being lost after neanic stages. (*Fig.* IX. of *Plate.*)

T. tenuitexta appears to be a further stage of *T. Damoniana* α, as its tuberculations are small, and further developed than in that form.

This type of umbonal marking is seen in the Cretaceous of India and South Africa in the forms described by Dr. Kitchin. Here, however, the flank markings are further modified by their junction into a V which, much as in *Goniomya*, occupies the whole flank.

Classification as based on Ornament.

The forms here described fall under Lycett's classification into two groups.

(*a*) The Undulatæ, comprising the Inferior Oolite to Corallian forms.

(*b*) The Gibbosæ, a sub-group of the Glabræ, including the Portlandian forms. [Bigot.]

It is, I think, permissible to class these two together in one great sub-division, the Undulatæ.

The more usual ancestry for the Gibbosæ is to describe them as degenerate Clavellates. The reasons given for doing this are :—

(1) Both Clavellates and Gibbosæ have concentrically marked areas.

(2) The Clavellate umbones are concentrically marked, the Gibbosæ type being derived from the Clavellate by loss of the clavellations.

(3) The ancestry of the Gibbosæ is unknown, and, although the umbones resemble *M. curvirostris*, yet it is impossible to get a Triassic type in the Portlandian without any intervening links.

Against this theory and for the junction of Undulatæ [Lyc.] and Gibbosæ may be said,

(1) That there is a fairly complete ancestry bridging over the gap between Triassic and Portlandian forms.

(2) That the Undulatæ agree with the Gibbosæ in umbonal markings in both flank and area.

(3) That the Clavellate umbonal flank markings, as I hope to show later, are not concentric.

TABLE SHOWING THE POSITIONS OF THE ABOVE-MENTIONED *Trigoniæ*.

RECENT			*T. pectinata*
MIOCENE			*T. acuticostata*
EOCENE			*T. subundulata*
CRETACEOUS			
PORTLAND	*T. tenuitexta*	*T. Damoniana* β	
	T. Damoniana α	*T. gibbosa*	
KIMMERIDGE			
CORALLIAN	*T. geographica*	*T. undulata*	
OXFORDIAN			
CORNBRASH	*T. detrita*	*T. undulata*	
FOREST MARBLE		*T. undulata*	*T. costata*
GT. OOLITE	*T. clytia*	*T. painii*	
INF. OOLITE	*T. v-costata*	*T. costatula*	*T. denticulata*
LIAS	*T. lingonensis*		
RHÆTIC			*M. emmrichi*
TRIAS	*M. curvirostris*		*M. lineata*
PRETRIAS	Pure concentric type		

Affinities of the Undulatæ [= *Undulatæ* (*Lyc.*) *and Gibbosæ*, *Lyc.*].

The closest connection of the Undulatæ appears to be with the Costatæ [Lycett]. It has been said that no Costate Trigonia has ever shown the faintest trace of concentric ornament on the area. There is, however, in the Manchester Museum a *T. denticulata* [Inf. Oo.] with pustulations on the marginal carina which agree in position and number with the ends of the flank costæ. Similarly arranged tubercles occur on the inner carina, and joining these two are regularly arranged rows of tubercles. A very young specimen from the Bradford clay, possibly *T. denticulata*, hints at the same arrangement.

From the *Rhætic*, of Bristol, comes *Myophoria emmrichi*, which has purely concentric flank markings and predominant radial markings on the area with a trace of concentric. This can only be seen in a few specimens, and even then with difficulty, as they are not well preserved.

In the *Trias* occurs *M. lineata* (*Fig.* II. of *Plate*), which, as described before, has pure concentric flank markings and predominant concentric markings on the area, showing the introduction of radial near the inner carina.

This traces the costate type of ornament back into Triassic times. Its later development takes place in the Tertiary epoch. The *Eocene* Australasian form, *T. subundulata*, shows pure concentric marking on the flank, except close to the marginal carina, where it becomes interrupted, showing the introduction of radial ornament.

The Miocene and recent Trigoniæ show tuberculate radial ornament over the whole flank, except for a small space below the umbo, near to the anterior edge of the shell. Here occur concentric lines, which at the back, meet

the radial ornament, running up to the umbo. The length of these concentric lines is gradually reduced by the introduction of radials.

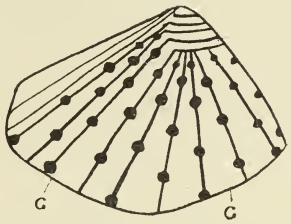

Fig. 1.

Diagram illustrating the umbonal and flank ornament of Recent and Miocene Trigonias. GG = a growth line.

This, then, gives the history of costate ornament from early Triassic or pre-Triassic times to recent; and shows the close connection between it and the Undulate types.

General Considerations on Ornament.

Beecher has shown that ornament is due to special body activity. As it is the mantle edge which secretes the shell in the Pelecypoda, the ornament must be due to extra activity of the mantle edge, as a whole, at intervals of time. Possibly these time intervals coincide with the variation of the seasons, the ornament being developed in summer when the body activity as a whole is augmented. The simplest kind of ornament will then be the *purely concentric.* (*Fig.* 3 A.)

The next stage to be developed will be that due to a mantle edge which is especially active at definite points. That is to say, the ornamentation will be still concentric, but will also be tubercular.

As shown by Beecher tubercles or spines are due to the intersection of two lines of ornament, and these

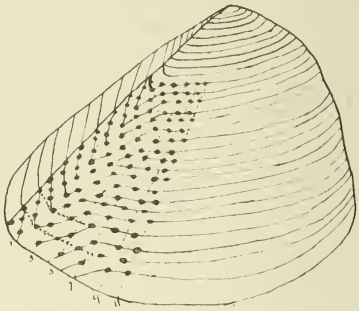

Fig. 2.

Diagram illustrating the origin of the undulate type of diagonal ornament.
x x x growth lines across the diagonal ornament.
1, 3, 5, 7 radii.

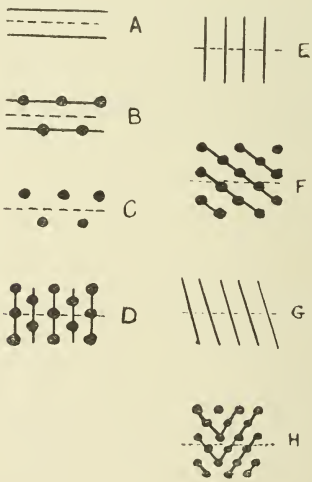

Fig. 3.

- - - - Growth lines.

Diagrams illustrating the development of ornament.

A. Concentric, due to intermittent activity of the whole mantle edge.

B. Tuberculate concentric due to intermittent activity of the whole mantle edge, augmented at certain spots.

C. Tuberculate due to intermittent activity of definite parts of the mantle.

D. Tuberculate radial due to continuous activity of the mantle at definite points, and augmented intermittently.

E. Radial, due to continuous activity of the mantle at definite points.

F. Tuberculate diagonal due to waves of activity passing round the mantle, and intermittently augmented.

G. Diagonal, due to waves of activity passing round the mantle.

H. Double diagonal, due to converging waves of activity, starting from opposite ends of the mantle.

tubercles will show the introduction of the radial element. This stage might be called *tuberculate concentric.* (*Fig.* 3 B.)

The development of these tubercles is due to the suppression of the concentric ornament along radial lines. The suppression occurs along each radius and may affect every concentric ridge, or alternate ridges only. In the latter case alternate concentric ridges are suppressed along alternate radial ridges only. In the Trigoniæ the latter rule holds, resulting in quincuncially arranged tubercles.

After this stage development is possible along two lines, either the concentric lines between the tubercles die out leaving simple pustular ornament, = *purely tubercular stage,* or the concentric lines die out, and the specially active spots become continuously active, giving radial lines which show the last stages of the concentric ornament in their pustulations, = *tuberculate radial stage.* These tubercles may die away leaving the *purely radial stage.* (*Fig.* 3 C, D, E.)

A still further stage of ornament is shown in the Trigoniæ, where the alternating turbercles join forming diagonals, these may be *tuberculate diagonal*, or *purely diagonal*, or even doubly diagonal (= V). The development of ornament then should be from concentric through radial to diagonal. (*Fig.* 3 F, G, H.)

The proof that this is so in the Trigoniæ, at least, is seen in the Undulatæ, where the double diagonal is developed by rejoining of tubercles in ephebic stages of individuals which, in early life, have pure concentric markings. It is also seen in the ontogeny of the modern forms, where the partial concentric umbonal markings are replaced in later stages by tuberculate radial ornament. Their phylogeny, as well as their ontogeny, also bears out this order of evolution of ornament, since the Eocene types

show almost pure concentric flank markings with only a hint of posterior development of radial, and the Miocene, like the recent, lose all trace of concentric markings after nepionic stages.

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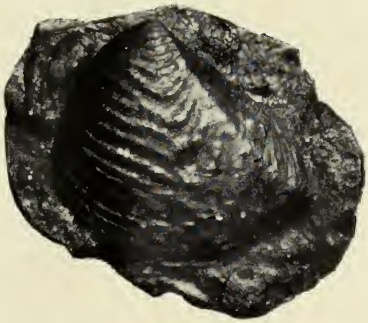

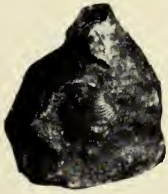

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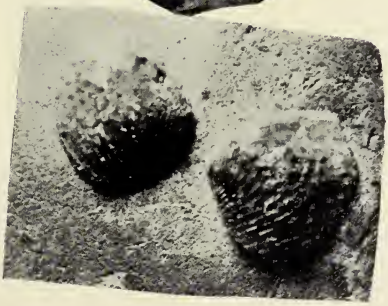

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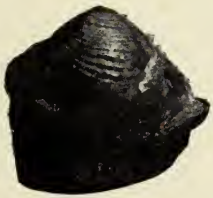

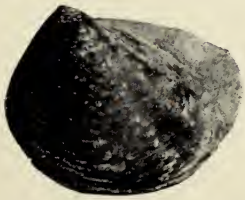

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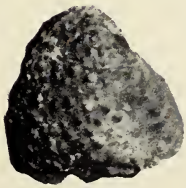

IX.

Fig. I. = *M. lineata* flank (M.M.).
,, II. = *M. lineata* area (M.M.).
,, III. = Cast from impression from the Stonesfield slate, labelled *T. Moretoni* (M.M.).
,, IV. Cast of young *T. gibbosa* Portland stone (M.M.).
,, V.
,, VI. } Growth stages of *T. gibbosa* from Portland stone (M.M.).
,, VII.
,, VIII. Adult form, labelled *T. gibbosa* (M.M.).
,, IX. *T. Damoniana* β (M.M.).

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XII. Some Physical Properties of Rubber.

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AND

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Introduction.

The demand for rubber for industrial purposes has led to an enormous increase in the supply of this material within the last few years. The applications of rubber in the arts are based on some one or other of its many physical properties, and it is now beginning to be recognised that more reliance can be placed in the indications given by physical tests on rubber as to its suitability for mechanical or electrical work than in those yielded by chemical analysis.

The true function of the physical tests in this connection would appear to be to deal with *effects*, while that of the chemical tests would be to determine the *causes* which produce these effects.

Our knowledge of the physical properties of this important material has not kept pace with its output for industrial purposes, and we are indebted mainly to two early members of this society—John Gough and J. P. Joule—for much of our knowledge of its curious properties.

Experimenting in 1802 John Gough[1] found that :—"By placing a slip of rubber in slight contact with the

[1] *Mem. Lit. & Phil. Soc. Manchester*, 2nd Series, vol. 1.

edges of the lips and then suddenly extending it he experienced a sensation of warmth arising from an augmentation of the temperature of the rubber, and then by allowing the strip to contract again he found that this increase of temperature could be destroyed in an instant."

In his next experiment he found that "If one end of a slip of Caoutchouc be fastened to a rod of metal or wood, and a weight be fixed to the other extremity, in order to keep it in a vertical position; the thong will be found to become shorter with heat and longer with cold."

In his third experiment he says, "If a thong of Caoutchouc be stretched, in water warmer than itself, it retains its elasticity unimpaired; on the contrary, if the experiment be made in water colder than itself, it loses part of its retractile power, being unable to recover its former figure; but let the thong be placed in hot water, while it remains extended for want of spring, and the heat will immediately make it contract briskly."

Joule[2] also observed the curious fact that a piece of indiarubber, softened by warmth, may be exposed to the zero of Fahrenheit for an hour or more without losing its pliability, but that a few days' rest at a temperature considerably above the freezing point will cause it to become rigid.

The remarkable series of experiments carried out by Joule on the thermo-dynamic properties of rubber will be referred to in detail in connection with the authors' experiments on the same subject.

Thermo-Dynamic Properties.

When a solid body is subjected to a tensile force, certain molecular changes take place, the mechanism of

[2] "Some Thermo-dynamic Properties of Solids."—*Phil. Trans.*, 1859.

which is not yet fully understood. If the body changes shape, a certain disturbance amongst the molecules is inevitable. The molecules probably become re-arranged, and if the tensile force is sufficiently great, certain groups of molecules may be broken up, whilst other groups will only be distorted, coming back to their original position when the stress is removed. Viewed from a rather different standpoint, these molecular changes may be divided into two classes, each producing its own characteristic phenomena, viz.:—

(*a*) A displacement of the position of the molecular groups relatively to their positions of equilibrium.

(*b*) A change in the dimensions of the intermolecular spaces.

Both of these effects take place simultaneously, and it is necessary to differentiate between them. The first of these, namely—the movement of the molecular groups past one another, is a cause of heating if we regard the effect as being of the nature of internal friction. In the second case, in order to overcome the attraction between the molecules, work has to be expended on them to increase their distance apart; this absorbs energy and is consequently a source of cooling.

When work is performed on a body due to a change of stress, this work must either go towards increasing the potential energy, or the kinetic energy of the body. The kinetic energy of the molecules re-appears as heat, and thus an alteration in the state of strain of a body results in a change in temperature. There is, however, a condition under which no thermal change takes place; this occurs when the whole of the work done goes towards increasing the potential energy of the body. If, during a given change of strain, the increase of potential energy is greater than the work expended on the specimen, then

the change will be accompanied by a cooling, since the extra energy must be produced at the expense of the kinetic energy of the body; or, in other words, at the expense of its heat.

In order to measure these thermo-dynamic effects quantitatively, it is necessary to know on what physical quantities they depend. The heat energy produced by the relative movement of the molecular groups past one another is proportional to the total work done upon the specimen in extending it, and, if the rate of extension with load is uniform, this is equal to $\frac{1}{2}We$, where (W) is the weight (added gradually) which is required to produce the extension, and (e) is the longitudinal extension. Chauveau[3] states that the second kind of internal work (change in the dimensions of the inter-molecular spaces) is simply a function of the distance traversed by the load, assuming that the changes of volume experienced by the body are proportional to its changes of shape. Work has to be done on the body in order to separate the molecules of which it is composed; in other words, its potential energy is increased. This is equivalent to saying it is a source of cooling, for it absorbs heat energy and converts it into strain energy or internal potential energy.

Experiment shows that, when rubber is subjected to tension, a cooling effect is first produced which decreases in value to zero, ultimately changing to a heating effect as the tension is increased. In compression, however, the inter-molecular spaces are diminished in size, and heating results from this cause as well as from the movement of the molecules past one another; these two effects are, therefore, additive, both producing heating.

[3] "Sur le mécanisme des phenomènes thermiques liés à la mise en jeu de l'elasticité du Caoutchouc." Paris, l'Académie des Sciences, Feb., 1899.

It is apparent that if, on the extension of rubber, first one effect and then the other is dominant, the resulting thermal effects will, at some point, undergo a change in sign, and for a certain extension there will be a neutral point or point of inversion where there will be neither heating nor cooling. At this point the heat energy engendered by the motion of the molecular groups relatively to one another, is equal to the potential energy gained on account of the enlargement of the intermolecular spaces. This point of neutrality, Chauveau states, exists both during extension and retraction, but not at the same tensions.

It is important to note that the two effects cannot be detected at the ordinary temperatures of the atmosphere, but are only found at low temperatures. Perhaps this is due to the fact that, at the higher temperatures, the weak type of molecular group breaks up at the first application of stress, or, it is possibly unstable altogether at the ordinary temperatures of the atmosphere. Under these conditions the extensibility of the rubber is much greater than at low temperatures, and it would seem that the positive work is in excess of the negative work done for all loads. As the temperature is lowered, a point is reached at which the two kinds of work done are equal in amount at the commencement of the application of load, and below this point the negative work would predominate initially.

The classic experiments of Joule on this subject were conducted at 6°C. and 7·8°C. in the case of unvulcanised and vulcanised rubber respectively, but it does not appear that he took any great care to keep his temperature constant, and it is quite possible that even a small variation in temperature would affect the result, since Joule says, "At temperatures a few degrees higher, the

reverse action with weak tensile forces did not take place, but that there was, on the contrary, a very slight heating effect." *Fig.* 1 is a diagram of his apparatus, reproduced from his original papers. The experiments performed by the authors were conducted at the temperature of melting ice, in order to secure the advantages of constancy which can be thus obtained.

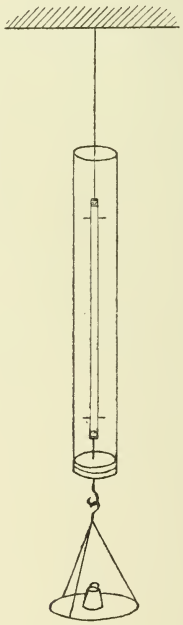

Fig. 1. Joule's Apparatus.

The method of measuring the temperature employed by the authors was substantially the same as that used by Joule, namely, by means of a thermo-junction inserted in a longitudinal slit near the middle of the specimen. The differences in detail consisted in the use of a single pair of copper and iron wires in the case of Joule, and ten pairs of copper-eureka junctions used with a sensitive

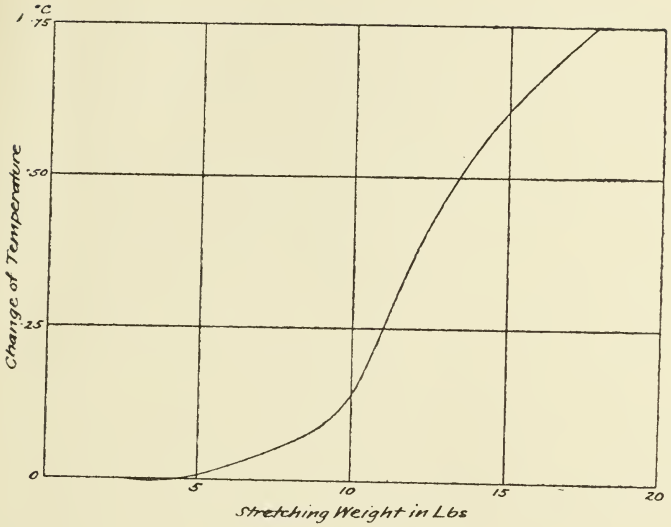

Fig. 2. Joule's Results.
Rubber $\frac{4}{10}$" square. Not vulcanised.

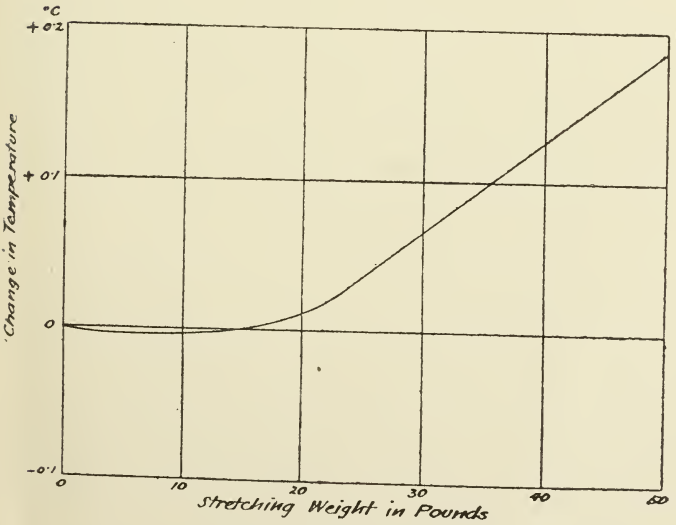

Fig. 3. Joule's Results.
Vulcanised Rubber. Section $\frac{3}{8}$" square. 1,452 grains per foot.

moving coil mirror galvanometer in the present instance. Typical examples of the results obtained by Joule are shown in *Figs.* 2 and 3, which are useful as indicating the magnitude of the effects referred to.

In the series of tests which were carried out by the authors, the specimens used consisted of 88% Para rubber, the remaining 12% being mineral matter of small extensibility. The test pieces were $\frac{1}{4}''$ square in cross-section and about 6″ long. The actual value of the length does not affect the result, since the total quantity of heat generated is directly proportional to the length used. The specimen was suspended inside a glass tube, which was surrounded by a vessel containing crushed ice to keep the temperature constant at 0°C. Owing to the very slow rate at which rubber conducts heat, it was found advisable to embed the specimen in a block of ice the night before the test, in order to cool it down to freezing point. In addition to this, the two ends of the containing tube were plugged with cotton wool to prevent the ingress of the outside air. Ten longitudinal incisions were made in the specimen at equal distances apart, in each of which was inserted a thermo-junction made of No. 40 S.W.G. copper and eureka wires soldered together. These ten thermo-junctions were connected in series and across the terminals of a sensitive moving coil mirror galvanometer, the movement of which was aperiodic, due to the gradual rise of temperature experienced by the thermo-junctions. Weights were attached, by means of a cord, to the lower end of the rubber specimen, and the resulting deflection of the galvanometer noted, the specimen being brought back to a condition of no load between each reading. Frequently a minute or so would elapse before the maximum deflection was registered, so that, except in the cases of the smaller differences of temperature, some heat would be lost by

radiation and other causes before the galvanometer came to rest.

It was found that a deflection of 1 mm. on the galvanometer scale was produced by a potential difference of 0·0241 micro-volts at its terminals. The thermo-electric value of copper and eureka being taken as 40 micro-volts per degree Centigrade, it was found that 1 mm. deflection corresponds to 0·000191°C.

The curve in *Fig.* 4 is a typical example of the thermal

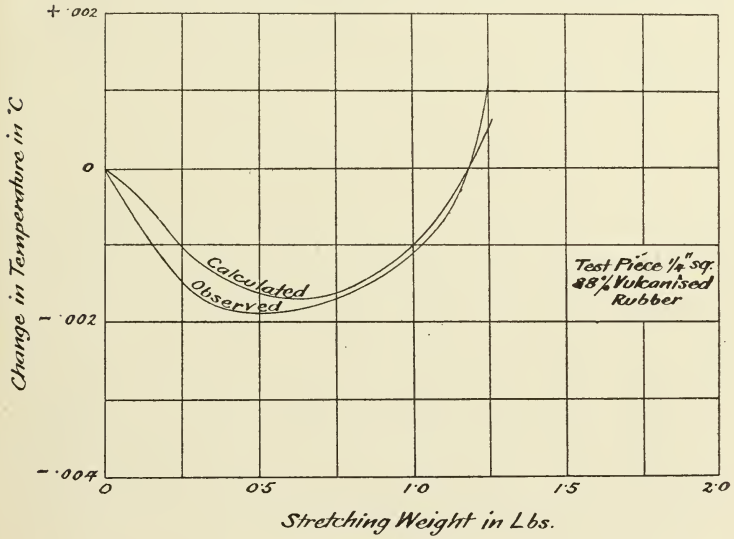

Fig. 4.

effects obtained by the authors, the neutral point, where neither heating nor cooling takes place, occurring at a tension of 19 lbs. per square inch. The other curve in the same figure is obtained by calculations based on Chauveau's hypothesis. The calculations are based upon a length of 1 cm.

Initial volume of 1 cm. length =0·4 c.c.
Specific Heat... ... ... =0·415 (Joule).
Specific Gravity ... ... =0·965 (Joule).

Ergs required to raise 1 cm. length through 1°C.

$$= 0{\cdot}415 \times 0{\cdot}965 \times 0{\cdot}4 \times 4{\cdot}2 \times 10^7$$
$$= 6{\cdot}73 \times 10^6.$$

It is here assumed that the specific heat and specific gravity remain unchanged on the application of tension. The work done in extension is equal to $(\frac{1}{2}We)$, where (W) is the tension applied gradually, and (e) is the elongation. This is equal to the work done in displacing the molecules relatively to one another, and is a cause of heating. The work done in enlarging the intermolecular spaces is equal to

$$K\frac{L_e\{L-(L_e-L)\sigma\}^2-L^3}{L^3},$$

where (σ) is Poisson's ratio, (L_e) the extended length, (L) the original length, and (K) a constant. This work increases the potential energy of the body, and is a source of cooling since it absorbs energy. Thus the effects of the two kinds of internal work are in opposition. If, therefore, it is known at what tension neither heating nor cooling occurs, it is possible to determine the numerical value of (K). This is done by equating the values of the two kinds of molecular work for the given conditions.

Therefore

$$\tfrac{1}{2}We = K\frac{L_e\{L-(L_e-L)\sigma\}^2-L^3}{L^3},$$

at the neutral point.

It is now possible to evaluate the above expressions for different tensions, and to calculate the temperature variations due to each particular load. The following table is an example of such a calculation :—

Stretching Weight in lbs.	Poisson's Ratio.	Extension in cms.	Positive Work Done in Extension in Ergs.	Negative Work Done in Increasing Inter-Molecular Spaces in Ergs.	Resultant Work Done in Ergs.	Temperature Change Calculated in °C.	Temperature Change Observed in °C.
0·125	0·23	0·020	600	3200	− 2600	− 0·0004	− 0·0006
0·25	0·122	0·039	2200	8700	− 6500	− 0·0010	− 0·0015
0·375	0·089	0·059	5000	14300	− 9300	− 0·0014	− 0·0019
0·50	0·071	0·076	8500	19300	− 10800	− 0·0016	− 0·0015
0·625	0·060	0·095	13300	24600	− 11300	− 0·0017	− 0·0017
0·75	0·054	0·114	19000	29800	− 10800	− 0·0016	− 0·0017
0·875	0·050	0·134	26100	35300	− 9200	− 0·0014	− 0·0013
1·00	0·047	0·154	34400	40800	− 6400	− 0·0010	− 0·0013
1·125	0·046	0·173	43300	45700	− 2400	− 0·0004	− 0·0006
1·25	0·045	0·193	53700	51000	+ 2700	+ 0·0004	+ 0·0015

In comparing together the calculated and observed curves in *Fig.* 4, it will be seen that the observed temperature difference is slightly greater than the calculated one for both positive and negative values. The difference between the two curves is, however, slight, particularly for stretching weights of 0·5 lb. and over. In the initial stage, when there is a fall of temperature, the observed curve falls away from the calculated one, but quickly comes into close agreement with it, remaining so throughout the remainder of the range. The heat radiated away or otherwise lost before the galvanometer reached its maximum deflection does not seem to have had any appreciable effect.

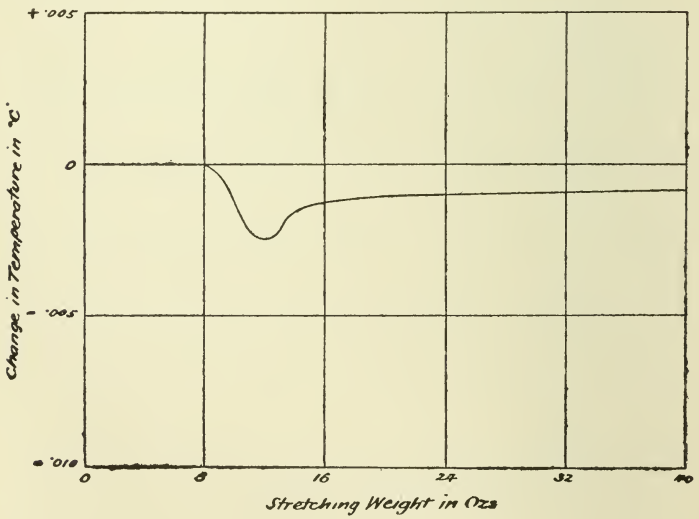

Fig. 5.

Spread Rubber Tape. Rested for 3 Years. Stretched at 0°C.

Fig. 5 shows the results obtained by testing in a similar manner a piece of spread rubber tape which had been rested for three years. The effect of age is indicated by

comparison with *Fig.* 4, the older specimen showing cooling over a greater range of stretching weights than the new specimen.

Expansion on Heating.

For the purpose of accurately measuring the expansion of rubber under conditions of varying temperature, the

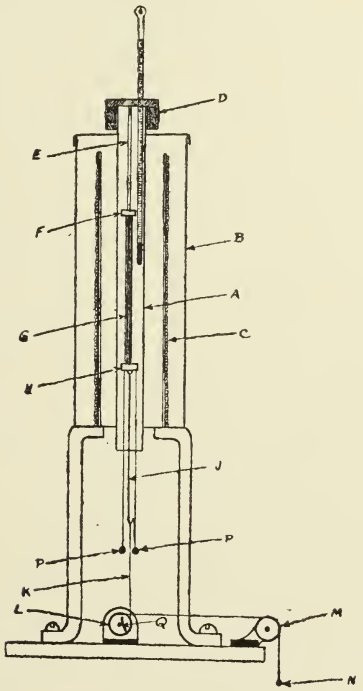

Fig. 6. Authors' Expansion Apparatus.

apparatus shown in *Fig.* 6 was designed. A copper tube 'A,' 14″ long and 1″ in diameter, was surrounded by an oil bath 'B' heated electrically by means of a coil of bare wire 'C' wound upon a wooden frame. Over the upper end of the tube 'A' was fixed a tightly-fitting cap 'D' well lagged with asbestos. From the centre of this cap a

brass rod 'E' depends, ending in the upper grip 'F.' The specimen 'G' is suspended vertically from this, the lower end being attached to the second grip 'H.' A thin glass rod 'J' extends from the lower grip to a point outside the heater, where it is attached to a cord 'K' passing round the pulleys 'L' and 'M.' The weight 'N' serves to keep the specimen taut without putting any further tension on it. In order to exert additional tension, two weights 'PP' are suspended directly from the lower grip, this method having the advantage of not subjecting the pulley 'L' to any severe stress. Two weights are here used in order to keep the lower grip in a horizontal position. A galvanometer mirror 'Q' is fixed on the axle of the pulley 'L,' and this reflects a beam of light on to a scale at a distance of two metres. In order to correct for the expansion of the apparatus itself, a copper strip, of similar dimensions to the test piece, was inserted in place of the rubber specimen. The apparatus was then heated up, and a curve of scale readings at different temperatures was obtained. The correction curve thus obtained was used when measuring the changes in length of specimens.

It has been frequently stated that rubber has a negative co-efficient of linear expansion, but, from results obtained by means of the above apparatus, it is clear that this co-efficient is positive in the case of vulcanised rubber under no load, if the specimen has *never been previously stretched.* If, however, the specimen is extended for a short period and allowed to rest for some time under no load, on the application of heat a contraction will be observed, being followed by an expansion. *Fig. 7* is illustrative of these points, the specimens used being 88% vulcanised rubber, the remaining 12% being mineral matter. The specimens used were $\frac{1}{4}''$ wide, $\frac{1}{16}''$ thick and about 6″ long, the length being accurately determined in

each case. The curve 'A' in *Fig.* 7 is that obtained from a virgin specimen, and shows an expansion throughout the whole range of the experiment, the co-efficient of linear expansion gradually increasing as the temperature was raised. In the case of the curve 'B,' the specimen was cut from the same sheet as 'A,' but was stretched to double its original length, and held in this condition for

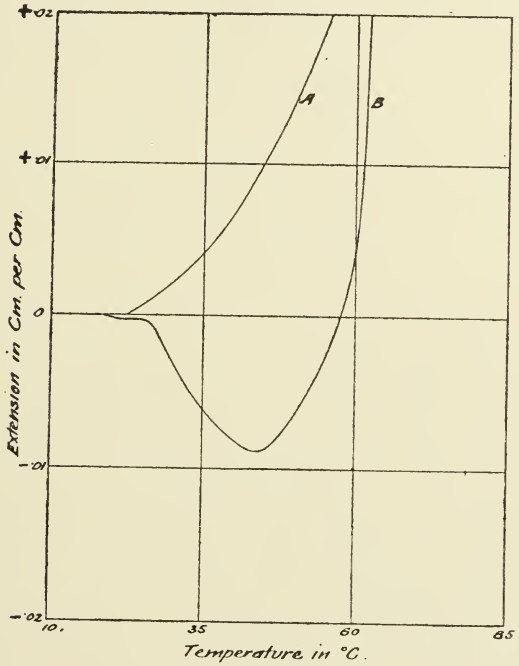

Fig. 7.

88% Rubber Vulcanised.
A = Virgin specimen.
B = Specimen stretched 100% for 5 minutes.

five minutes. The tension was then removed, and the specimen allowed to rest for a period of three or four hours, in order to reduce the effects of sub-permanent set. The test was then proceeded with and the curve 'B' was

obtained. This shows a marked decrease in length of the specimen up to a temperature of about 45°C., after which the rubber began to expand, reaching its original length at a temperature of 58°C. Above the latter temperature the expansion became more and more rapid as the temperature was raised.

Now rubber is more extensible at lower than at higher temperatures, and consequently the increase in length is less when the temperature is raised. The deformation after the stress is removed is also less. Thus when the specimen 'B' was heated, the rubber at first attempted to regain its original shape, and a contraction resulted. After a time, however, the natural expansion begins, as is indicated by the upward turn of the curve in *Fig.* 7 at 45°C.

Experiments were also conducted on spread rubber tape which had been allowed to rest in an unstressed condition for about three years. The specimens were cut to a uniform width of $\frac{1}{4}''$, the average thickness being 22 mils. During this period of rest, the rubber had become opaque and hard, a condition which can be destroyed by warming gently, the rubber apparently returning to its original plastic and translucent condition.

On the first application of heat to a specimen of this rested rubber tape, an expansion was noted, as shown in *Fig.* 8, the expansion curve bending upwards, indicating a gradual increase in the linear co-efficient of expansion. This continued until a temperature of about 33°C. was attained. At this juncture a sudden increase in the rate of expansion took place, the curve again approximating to a straight line, but with a greatly increased slope. On cooling again the specimen rapidly contracted for a time, after which its length became approximately constant, as

is indicated by the cooling curve in the diagram. This new constant length was slightly greater than the original length. It would seem, therefore, that rubber possesses the property of "creeping" under changes of temperature in the same way that lead does.

A further test was made on a similar specimen of rested rubber tape under a constant load of 50 grams, which corresponds to a tension of 16·7 lbs. per square inch. (*Fig.* 9.) This test extends throughout the same range of temperature as the previous one, so that the effect of

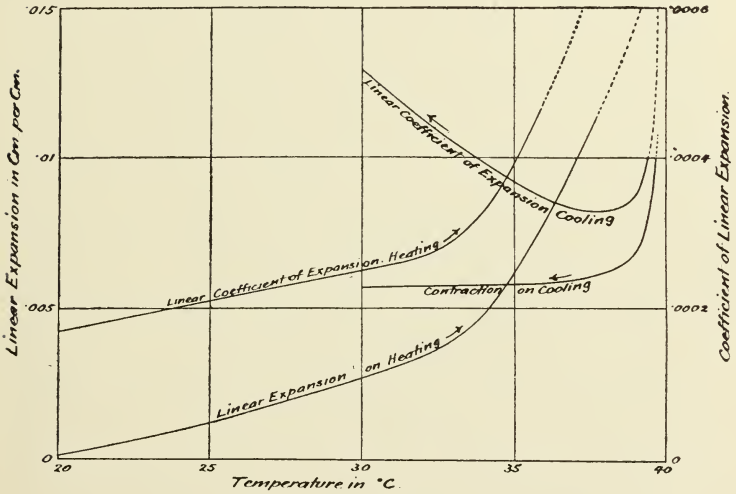

Fig. 8.

Spread Rubber Tape Rested for 3 Years.

tension is clearly illustrated by comparing the two with one another. It will be seen that the curves are very similar up to a temperature of 33°C., at which point a sudden bend occurs in both curves. The specimen under tension now begins to expand much more rapidly than the other one, the curve becoming very steep indeed, the rubber apparently softening and gradually giving way.

These tests point to the fact that the previous history of the specimen is of great importance in determining the strain resulting from a given stress.

Further experiments with a greater load on the specimen showed that a small initial contraction followed by a rapid expansion resulted on the application of heat. On further increasing the load it was found that, under loads sufficient to give an initial extension of the order

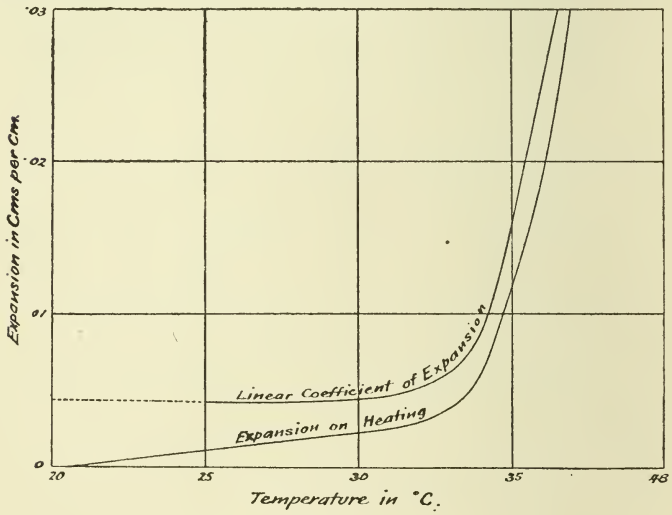

Fig. 9.

Spread Rubber Tape, Rested for 3 Years. Tested under Tension of 50 grams. = 16·7 lbs. per sq. inch.

of 100%, the application of heat was found to lead to contraction only. There is evidently, therefore, some critical load up to which the application of heat results in expansion and beyond which contraction occurs. In the discussion following the reading of this paper Professor Gee pointed out that Schumlewitsch[4] had come to the

[4] Schumlewitsch. "Ueber den Kautschuk," *Vierteljahrssch. der Naturforsch. Gesell. Zurich.* 1866.

same conclusion in 1866. A reference to his paper confirms the authors' results as to the extension and contraction on heating under tension, and they hope to extend their observations on this subject.

The Mechanical Hysteresis of Rubber.

If a rubber test piece be subjected to a gradually increasing load to some point below the breaking load of

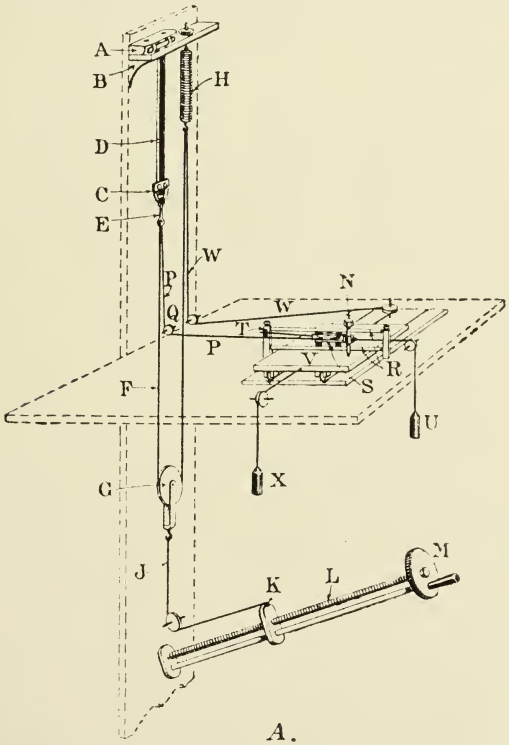

Fig. 10. Rubber Testing Machine.

the specimen, and on reaching this point the load be reduced at the same rate to zero, it will be found that the length of the test piece has been increased by a certain amount of sub-permanent set.

If the relation between load and extension throughout the above cycle of operations be plotted, it will be found that an area is enclosed between the extension curve and the retraction curve which represents the work done in the rubber itself.

The authors have employed the mechanical hysteresis of rubber, above referred to, as an index to its quality, and have designed a machine in which the relation between the load and extension can be automatically recorded throughout the test. The machine is shown diagrammatically in *Fig.* 10. The test piece having been secured in the grips, the fixed grip 'A' is mounted on the pins on the bracket 'B,' and the movable grip 'C,' which depends from the specimen 'D,' is connected by the hook 'E' to the cord 'F,' which passes round the floating pulley 'G' to the helical spring 'H.' The load is applied and withdrawn by the up and down traverse of the pulley 'G,' which is effected by means of the cord 'J,' attached to a nut 'K,' which is moved along the guide by the screw 'L', actuated by hand at 'M.'

The relation between load and extension is charted in the following way :—The grip 'C,' the movement of which represents the extension, is connected to the pencil carrier 'N' by the thread 'P' which passes over the pulley 'Q.' The pencil carrier, which moves between the guides 'R,' contains a pulley 'S,' round which the thread 'P' passes to a stop 'T' (which is adjustable as to position), where it is made fast. The movement of the pencil is thereby reduced to one-half of the extension of the specimen. If necessary, this movement may be again reduced by one-half by the introduction of a light floating pulley in the thread 'P.' The movements of the pencil carrier are controlled by the counter-weight 'U.' Beneath the pencil carrier, and moving at right angles to it, is a light table 'V,' on which the chart

paper is fixed ; the movement of this table is directly proportional to the load as it is attached to the spring ' H ' by means of the thread ' W.' The movement of the table is controlled by the counter-weight ' X.'

Fig. 11 shows a reproduction of an actual diagram,

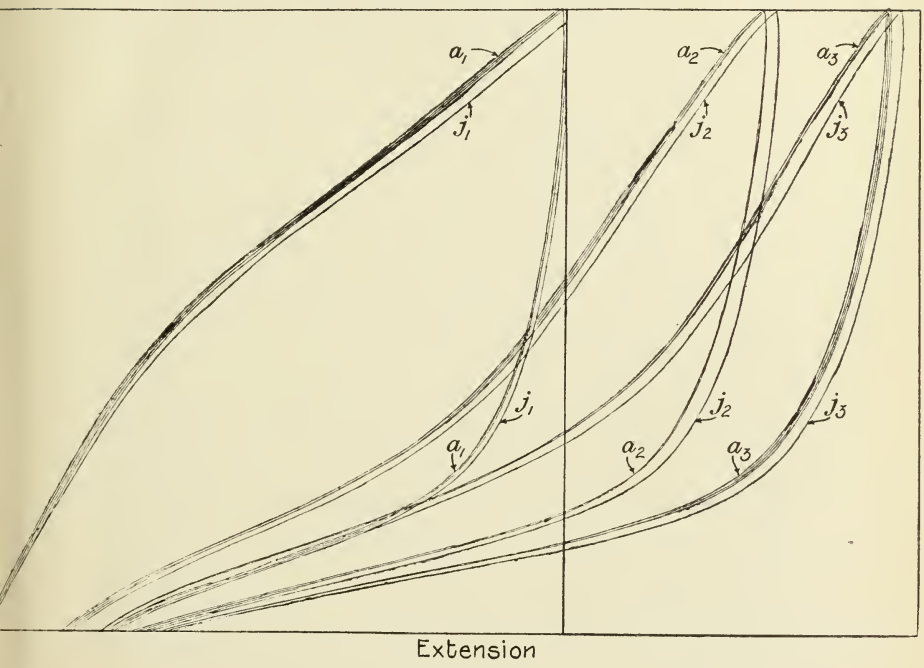

Fig. 11.

Reproduction of Actual Diagram showing Constancy of Results obtained with the *Hysteresis* Machine.

Test-pieces cut from 2,500 megohm cable covering, and put through three cycles of extension and retraction.

Curves a_1, a_2, a_3 are made up of four lines, each due to a separate test-piece.

Curves j_1, j_2, j_3 due to a single test-piece cut from the same cable covering as the others, but containing a longitudinal joint.

showing the constancy of the results obtained with the hysteresis machine.

The following laws are deduced from the results obtained with this machine :—

(1) The load, expressed in pounds per square inch of the original cross-sectional area of the test piece, is constant for a given extension within certain limits.

(2) The work done in extension, in retraction, and in the rubber itself, is proportional to the cross-sectional area of the test piece within certain limits.

(3) The areas of the hysteresis loops become constant after about the 6th cycle.

(4) The increments of extension, obtained on subjecting the specimen to a series of cycles of extension and retraction with a given maximum extension, follow a logarithmic law with respect to the number of the cycles.

In conclusion, the authors wish to express their thanks to the Principal and Committee of the School of Technology, Manchester, to Messrs. F. Shaw, B.Sc., and J. Davies, for assistance in carrying out the experiments.

XIII. The Manner of Motion of Water Flowing in a Curved Path.

By Prof. A. H. Gibson, D.Sc.

University College, Dundee.

Received January 13th, 1911. Read February 7th, 1911.

In announcing the results of his classical experiments on "The Two Manners of Motion of Water," before the Royal Institution of Great Britain,* Osborne Reynolds instances "curvature with the velocity greatest on the outside of the curve" as one of the circumstances conducive to direct, steady, or stream-line motion in a stream or jet of water; and "curvature with the velocity greatest on the inside of the curve" as conducing to sinuous motion or eddy formation.† The experiment on which this conclusion was based was carried out by means of a cylindrical vessel filled with water. This was allowed to come to rest, after which a colour band of aniline dye was introduced diametrically across the vessel, and the latter was rotated slowly on its axis. To quote its author: "At first, only the walls of the cylinder move, but the colour band shows that the water gradually takes up the motion, the streak being wound off at the ends into a spiral thread, but otherwise remaining still. When the spirals meet in the middle the whole water is in motion, but the motion is greatest at the outside, and is therefore stable. The vessel is stopped and gradually stops the water, beginning at the outside. If the motion remained steady

* *Proc. Roy. Institution of Great Britain*, 1884. Also "Scientific Papers," vol. 2, p. 153.

† "Scientific Papers," vol. 2, p. 157.

the spirals would unwind and the streak be restored. But the motion being slowest at the outside against the surface, eddies are formed, breaking up the spirals for a certain distance towards the middle, but leaving the middle revolving steadily." *

Various phenomena, of common occurrence in hydraulic work seem, however, to be at direct variance with the opinion expressed above. Among these may be instanced the behaviour of water in a free vortex, and at the discharge side of a sharp-edged orifice in a thin plate. In a free vortex, produced by the discharge of water through a central hole in the bottom of a circular vessel, the motion is readily shown, by colour bands, to be steady throughout, although here the motion is everywhere greater at the inside of the curved path of a filament. Again, on the discharge side of a sharp-edged orifice in a thin plate, during the gradual convergence of the stream tubes to the vena-contracta, the filaments, other than the central one, follow curved paths with the pressure gradually increasing, and the velocity diminishing towards the interior of the stream, and therefore as the radius of curvature increases. Yet such motion is essentially steady and non-sinuous. Another somewhat analogous case is found where a steady jet impinges on a solid surface, or where two steady jets, moving in the same straight line but in opposite directions, meet. In each case the motion after impact is steady, however high the velocity. Here again, owing to the change in the direction of flow following impact, the outer layers, while this change is being effected, are moving in curved paths with the velocity greatest at the outer (nearer to the centre of curvature) portion of the curve. Experiments on the flow of water around bends in piping also show that the velocity is greatest at the inside of

* "Scientific Papers," vol. 2, p. 162.

the bend, and indicate further that the loss of energy in the bend itself is no greater, but on the contrary is somewhat less, than the loss in the same length of straight pipe.* This indicates that the stability is increased by changing straight line motion into motion in a curved path, with the velocity greatest at the inside of the curve.

In view of the apparent discrepancy between the conclusions to be drawn from these results, and those commonly accepted, further experiments, somewhat on the lines originally adopted, have been carried out by the author. In the first of these a cylindrical drum, 4 inches in diameter and 12 inches long, is mounted so as to be capable of rotation about a vertical axis fixed centrally in a cylindrical vessel 12 inches in diameter and 12 inches high. The drum being in position the vessel is filled with water. When this has come to rest a colour band is introduced radially across the space between the drum and the vessel, after which the drum is rotated slowly. The colour band shows that the water gradually takes up the motion, the streak being gradually wound into a spiral thread without any trace of eddy formation. When rotation is stopped the water in contact with the drum is brought to rest, and eddies are formed breaking up the spiral for a certain distance towards the outside of the vessel. The phenomena are therefore identical, whether, as in Reynolds' experiment, the outer layers, or, as in the present experiment, the inner layers, are originally rotating the faster.

In the second experiment, the same drum was mounted with its axis vertical and central, over an orifice pierced in the bottom of a cylindrical vessel 2 feet in diameter. Water is supplied to the latter through a volute, opening

* Brightmore. *Proc. Inst. Civil Engineers*, vol. 169, 1906-7, pt. iii., p. 334.

out of its side. An approximation to a free vortex is formed by discharge through the central orifice, the velocity increasing as the radius diminishes, and attaining its maximum value within a short distance of the periphery of the central drum. Aniline dye was introduced at different points in the vessel, and by its behaviour shows: (1) That the motion is unstable around the periphery of the outer vessel. (2) That the eddies formed at this periphery die out as the centre is approached, and that the motion, except in the neighbourhood of the inner and outer boundaries, is essentially stable. (3) That the motion is unstable around the periphery of the inner drum, but that this instability is confined to the immediate neighbourhood of the periphery, the eddy formation being confined to a cylindrical annulus, whose radial thickness did not in general exceed one-quarter of an inch.

The conclusions which would appear to be justified as a result of these experiments, and of those previously mentioned, are (1) that whenever flow takes place past a curved solid surface, whether this is exposed to water on its concave or its convex side, the motion, except for the slowest velocities, is unstable; and (2) that in the fluid itself curvature with the velocity greatest on the *inside* of the path tends to stability, while curvature with the velocity greatest at the *outside* of the path tends to instability. The fact that in the original experiment the eddy formation noted at the periphery of the containing vessel when the latter was stopped, did not extend throughout the whole mass of fluid, is doubtless due to the extremely low velocities obtaining near the centre of the fluid. However inherently unstable the type of motion may be, a certain critical velocity is necessary in order that this may be manifested, and under the conditions of the experiment, the actual velocity was extremely small.

Another fact which the experiments appear to indicate, is that the tendency to eddy formation in the relative motion of a fluid and a solid surface is greater, for a given relative motion, when the fluid as a whole is moving past a stationary surface, than when the surface is moving through still fluid. This receives indirect confirmation from experiments by Stanton, Beaufoy, Froude, Dubuat, and Morin, on the resistance of plane surfaces when moving through still water, or when held stationary in a moving stream. The results indicate that for a given relative velocity of plane and water, the resistance is greater in the latter case. A possible explanation of this would appear to lie in the fact that in the former case, *i.e.*, with a moving plate in still water, the fluid, except in the immediate vicinity of the moving surface, is in a stable condition, and any eddy projected from the neighbourhood of the surface will be damped with a minimum disturbance of the surrounding fluid. On the other hand, with a body of fluid in motion, even if the motion remote from the centre of disturbance is stable, the balance of stability is less than in the former case, and any slight disturbance will have more widely reaching effects, and will lead to an increased loss of energy.

Manchester Memoirs, Vol. lv. (1911), *No.* **14**.

XIV. On the Periodic Times of Saturn's Rings.

By Henry Wilde, D.Sc., D.C.L., F.R.S.

Received and Read April 25th, 1911.

In my paper on the "Origin of Cometary Bodies and Saturn's Rings," read before the Society in October last,* a new determination was made of the periodic times of the rings, based on the commonly accepted distance of Mimas, 3·36 Saturnian units. This element of the orbit was derived from observations made by Herschel and subsequently adopted by all astronomical writers.

Recent observations of American astronomers with more powerful telescopes and under more favourable conditions, have reduced the distance of Mimas from the planet to 3·16 units, with the consequent increase in the times of rotation of the rings.

The difference between the older and later determinations is sufficiently large to induce me to place on record for comparison the results computed from both observations and Kepler's third law, as shown and demonstrated in the following tables :—

Distances of Mimas = 3·36 and outer edge of ring A = 2·3.

(1) $3{\cdot}36^3 : 2{\cdot}3^3 :: 22\text{h. } 37^2\text{m.} : x = 12\text{h. } 48^2\text{m.}$ for A.

Distances of Mimas = 3·36 and inner edge of ring C = 1·27.

(2) $3{\cdot}36^3 : 1{\cdot}27^3 :: 22\text{h. } 37^2\text{m.} : x = 5\text{h. } 15^2\text{m.}$ for C.

Distances of Mimas = 3·16 and outer edge of ring A = 2·3.

(3) $3{\cdot}16^3 : 2{\cdot}3^3 :: 22\text{h. } 37^2\text{m.} : x = 14\text{h. } 4^2\text{m.}$ for A.

Distances of Mimas = 3·16 and inner edge of ring C = 1·27.

(4) $3{\cdot}16^3 : 1{\cdot}27^3 :: 22\text{h. } 37^2\text{m.} : x = 5\text{h. } 45^2\text{m.}$ for C.

* *Manchester Memoirs*, vol. 55, 1910-11.

May 8th, 1911.

The unit distance, 3·16, for Mimas necessarily involves the correlative reduction of the distances of the other Saturnian satellites, as now set forth in the tables of these elements published by American astronomers.

That Saturn's rings are ejectamenta from the interior of the planet is further evident from the fact that no causal connexion subsists between their times of rotation and that of the planet itself, as the inner edge of the ring C has a periodic time of only 5 hours 45 minutes, while the axial rotation of Saturn is 10 hours 13 minutes.

The same conclusion may also be drawn with reference to the origin of the two satellites of Mars, as Phobos has a period of only 7 hours 39 minutes, while the axial rotation of the planet is 24 hours 37 minutes.

The comparative minuteness of these bodies, which are estimated to be less than 10 miles in diameter, indicates them as ejectamenta rather than the successive condensations of a nebular substance surrounding the planet.

Saturn's dusky ring and the inner satellite of Mars are the only bodies in the solar system that revolve in a shorter time than their primaries.

ELEMENTS OF SATURN'S RINGS.

Mimas = 3·16.

Rings.	Distance from centre of Saturn.		Time of Revolution.	
	Sat. Units.	Miles.	h.	m.
Exterior A. ...	2·30	84,937	14	4
Breadth	0·26	9,602	„	„
Mid-breadth ...	2·17	80,138	12	51
Interior A. ...	2·04	75,337	11	44
Interval	0·07	2,585	„	„
Exterior B. ...	1·97	72,752	11	8
Breadth	0·47	17,357	„	„
Mid-breadth ...	1·735	64,073	9	12
Interior B. ...	1·50	55,395	7	23
Exterior C. ...	1·50	55,395	7	23
Breadth	0·23	8,493	„	„
Mid-breadth ..	1·385	51,148	6	33
Interior C. ...	1·27	46,901	5	45
Ball Space ...	0·27	9,971	„	„
Sat. Ball ...	1·00	36,930	10	13
Mimas	3·16	116,698	22	37

Manchester Memoirs, Vol. lv. (1911), *No.* **15**.

XV. Studies in the Morphogenesis of certain Pelecypoda. (3) The Ornament of *Trigonia clavellata* and some of its Derivatives.

By Margaret Colley March, B.Sc.

Read March 21st, 1911. *Received for publication April 4th, 1911.*

The original title of this paper was "The systematic position of *Trigonia irregularis*, *Trigonia scarburgensis*, *Trigonia dædalea* and *Trigonia literata*." It was abandoned for two reasons, *firstly* because it was too cumbersome, *secondly* because that part of the paper appeared only at the very end, and in a seemingly incidental manner. I should like to point out that the curious ornament of these forms and their relationships led to the working out of that part of the paper which, in writing, had to come first.

The Ornament of T. clavellata.

The valves of this species have been described by Lycett* as having "rows of tuberculated costæ, at first oblique, but the later-formed few become more horizontal, or more nearly in accord with the directions of the lines of growth, so that the greater number of rows reach the anterior border in the form of attenuated or sub-tuberculated varices; the posteal extremities of the rows approach the carina at an angle which is greater than a right angle..."

The rows of ornament are diagonal, since they cut both radial lines and growth lines. They are formed of

* Lycett, 1875. "Trigoniæ," *Pal. Soc.*, p. 19.

quincuncially arranged tubercles. Indeed this arrangement alone gives a semblance of diagonal ornament without any fusion of tubercles, or development of inter-

Fig. 1*a*.

Apparent development of diagonal rows owing to quincuncial arrangement of spots.

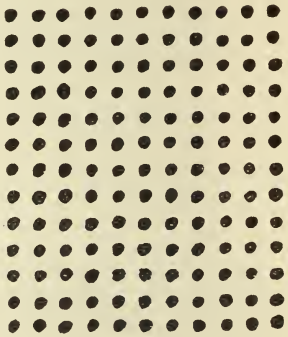

Fig. 1*b*.

Shows the apparent development of straight lines of ornament from the ordinarily arranged tubercles.

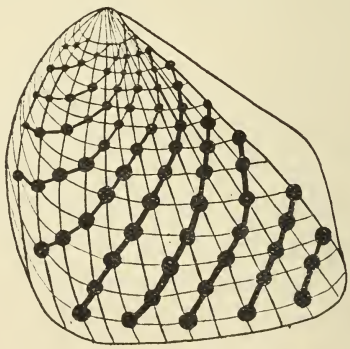

Fig. 2.

Showing the quincuncial arrangement of the tubercles of *T. clavellata*.

mediate ridges, as is seen in *Fig.* 1*a*, where the lines appear to run diagonally, not straight as they do in 1*b*.

The proof of this quincuncial arrangement is obtained by joining up the tubercles next to the marginal carina, then those of the second row, and so on. If, after this has been done, a growth line which cuts a tubercle is traced across the shell, it will be found to cut tubercles on alternate radii only, as in *Fig.* 2.

The diagonals do not form straight lines. The deviations from straightness occur in two places.

Firstly. The posterior upper and larger part of the diagonal is slightly bent. This is much less clearly marked in late stages.

Secondly. Near the anterior edge the lines of ornament bend sharply upward so as to approach a horizontal position. This bend only occurs in those diagonals which reach, or nearly reach, the anterior margin.

These curves vary in intensity with the individuals. They are most marked in those specimens which are elongated, and laterally compressed, and have the ornament carried right up to the anterior margin.

Relation between Growth and Variations in Ornament.

The first bend appears to be due to the divergence of the radials. *Fig.* 3*a* represents diagonals of a figure with such radii. As the radials diverge the figure enclosed by them and the concentrics becomes a trapezoid, at the angles of which the tubercles lie, and whose diagonals form the line of ornament. *Fig.* 3*c* gives ABCD and DEFG as such trapezoids. The elongation of the base GF of the figure DEFG forces its diagonal DF out of the line of AD, the diagonal of ABCD.

This appears to be the cause of the bending of the upper parts of the diagonals of *T. clavellata.*

The absence of this bend in the later-formed lines may be due to the fact that the divergence of the radials decreases as full growth is reached.

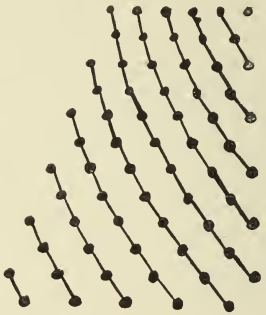

Fig. 3a.
Showing bending in diagonals due to divergence of radii.

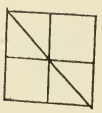

Fig. 3b.
Showing straight diagonals where the radials are parallel.

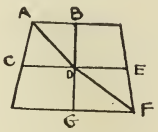

Fig. 3c.
Showing the cause of bending where the radials diverge.

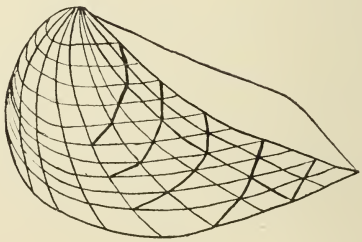

Fig. 3d.
Showing bending due to divergence on an actual specimen.

Naturally this curve is better developed in elongated forms, as in them divergence is greater, as the difference of length in early and late stages is more marked.

The *second* and more accentuated bend marks the introduction of that part of the ornament which more nearly approaches the concentric, that is, follows more closely the line of growth. It occurs in that part of the flank in which the growth lines bend sharply upward to run parallel to the anterior edge, and where they are also rapidly closing up. This upward bend causes the concentric ornament and consequently the tubercles in that part of the shell which it affects to be carried upward. So that if A, B, C, D, E & F represent the tubercles of a diagonal row above the bend, and G the tubercle after the beginning of the bend upwards, G will not lie in the straight line AF, but slightly above it in such a way that

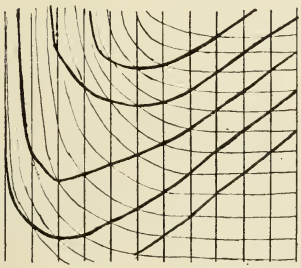

Fig. 4.
Showing the development of apparently concentric diagonals, owing to uplift.

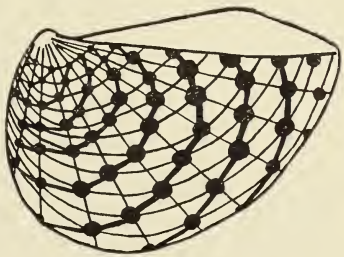

Fig. 5.
Showing a double bend on an elongated *T. clavellata.*

the line will have to bend up to reach it, as in *Fig.* 2 and also in *Fig.* 4. The continued uplift may cause the succeeding tubercles of a diagonal row to lie at the same level as, or even above their predecessors, thus forming a horizontal, or even an upcurved line which forms an acute angle with the upper portion. If the uplift is great enough apparent concentricity may be attained by the diagonals.

In laterally wide animals the uplift occurs on the anteriorly-faced part of the flank, where the ornament usually dies away. The diagonals, therefore, do not come into the region of the uplift and so lose this marked bend. It is also absent in any individuals where the ornament is not continued to the edge.

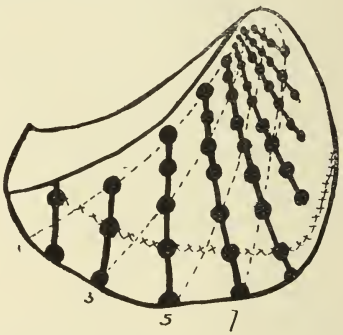

Fig. 6.

T. navis. Showing straight diagonals.

It is clear, therefore, that the deviation from the straight diagonal in the clavellate type of ornament is dependent on the relative growth of the parts of the shell and also on the form of the animal.

Some Causes of Variation in Form in Pelecypoda.

As has been shown for the British fresh-water *Unionidæ** the form of the individual is exceedingly sensitive to environment. The *length* of the shell varies with rate of the current and the consistency of the mud in which the animal lives. The *width* of the animal varies with the composition of that mud.

These *Unionidæ* live in very diverse surroundings and are probably a decadent group. These two facts make their range of variation great. Comparing

* March, 1911. "Studies in the Morphogenesis of certain Pelecypoda. (1) Prel. Note on the variation of *U. pictorum*, etc." *Manchester Memoirs*, vol. 55, No. 8.

them with the *Trigoniæ*, the range of variation in the latter must be far smaller in *Jurassic* forms at least. This, however, does not entirely remove these factors from affecting the *Trigoniæ*, though it lessens their apparent effect. One factor is probably removed, and that is current action.

Recent *Trigoniæ* are found from about 10 to 20 fathoms, a depth at which currents would not be felt. There is no reason to suppose that *Jurassic Trigoniæ* lived in shallower water, so that their elongation cannot be due to current action. It may be due to the consistency of the mud in which they lived. This supposition is supported by the fact that the elongated forms occur in the Oxfordian and Kimmeridgian clays. These were formed under similar conditions from heavy muds, which would tend to retard anterior growth, and accelerate posterior growth, of any animal crawling through them. It is true that *T. juddiana* is found in the same formation. It occurs, however, locally, and may be due to local conditions, as are short forms among otherwise elongated individuals in the *Unionidæ*. It might, perhaps, even be due to local conditions of food supply, which, in doing away with the necessity for much movement, would reduce the effect of the environment on the shell.

A tendency towards the development of an equilateral form would be due to the assumption of some habit, which, more or less, equalized the effect of the environment.

Such a form in the modern *Trigoniæ* and in *Pecten* is associated with the fact that they do not crawl but leap. Some such an equalizing habit must have been adopted by *T. dædalea* and its allies.

This variation in form and consequent variation in ornament may account for many of the so-called specific

differences. The variation in ornament, if worked out with its relation to shape, may show relationships of forms whose positions have hitherto been considered doubtful.

Extreme Types of Ornament.

(*a*) *T. irregularis.* This is an elongated Kimmeridgian and Oxfordian form. The extreme deviation of the

Fig. 7a.

Showing the apparent lines of ornament on *T. irregularis.*

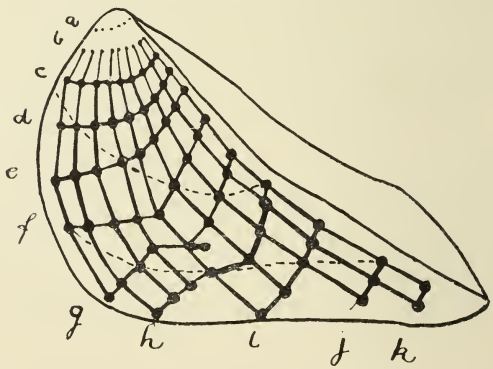

Fig. 7b.

Showing the true lines of ornament on *T. irregularis.*

posterior radials and the well-marked uplift of the anterior concentrics result in the marked bending and even breaking of the lines of ornament.

This bending or breaking is only seen in the middle part of the valve, because the extreme deviation of the radials is not felt by the upper diagonals, and the uplift not by the lower.

Fig. 7a represents a specimen of *T. irregularis* with its ornament interpreted according to the usual method, and showing at + the introduction of an apparently extra, intercalated row.

Fig. 7b represents the same specimen, with its tubercles joined up on their right radials. Here no intercalated row is visible, as the radials show that it belongs to the upper part of row *f*.

Fig.	*7a*	row	*a* =		row *a*	in	*Fig.*	*7b.*
,,	,,	,,	*b* =		row *b*	,,	,,	,,
,,	,,	,,	*c* =		row *c*	,,	,,	,,
,,	,,	,,	*d* =		row *d*	,,	,,	,,
,,	,,	,,	*e* =		row *e*	,,	,,	,,
,,	,,	,,	+ =		ant. end of row *f*	,,	,,	,,
,,	,,	,,	*f* =	ant. end of row *g* and post. ,, ,, row *f*		,,	,,	,,
,,	,,	,,	*g* =	ant. end of row *h* and post. ,, ,, row *g*		,,	,,	,,
,,	,,	,,	*h* =		post. end of row *h*	,,	,,	,,
,,	,,	,,	*i* =		row *i*	,,	,,	,,
,,	,,	,,	*j* =		row *j*	,,	,,	,,
,,	,,	,,	*k* =		row *k*	,,	,,	,,

The curious type of ornament is then due to the form, and, if the form is merely ecological, *T. irregularis* must lose its place as a species and become merely a variety of *T. clavellata.*

A similar reading may be taken for the ornament of *T. scarburgensis*, etc.

T. dædalea. This species is characterised by its curious, flattened tall form, and the apparently double set of ornament.

The shape of the animal accentuates the curve of the growth lines. The flatness of the valves brings this curve entirely on to the laterally-faced part of the flank, as there is no anteriorly-faced part for them to run upon. The diagonals get the full benefit of the marked up-curve,

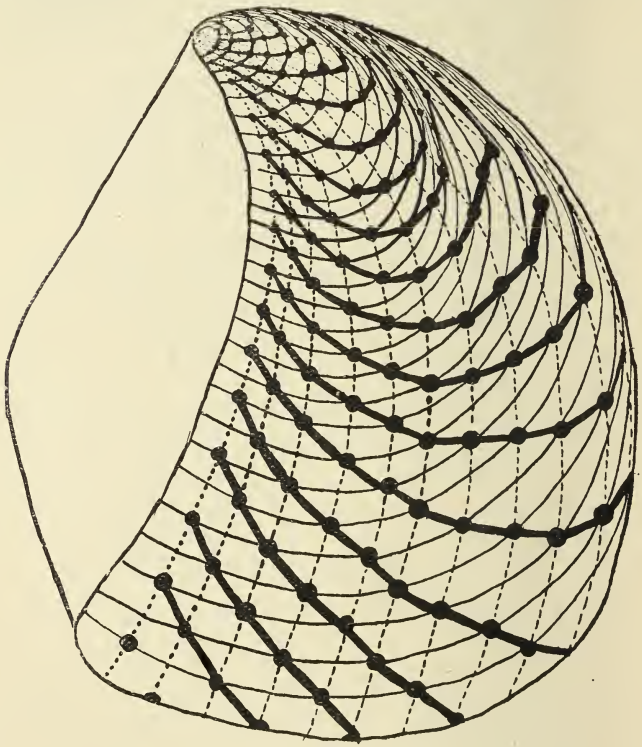

Fig. 8*a*.

Showing the effect of growth lines similar to those of *T. dædalea* on diagonals.

because they are continued to the anterior edge of the valve.

The effect of growth lines, similar to those of *T. dædalea*, on diagonals is seen in *Fig.* 8*a*. The up-curve becomes marked about the middle of the flank. In

the umbonal region the anterior and posterior parts of the line meet at an acute angle and form a V, the anterior limb of which is approximately concentric. A similar bend

Fig. 8b.

Showing the apparently double set of ornament in *T. dædalea*.

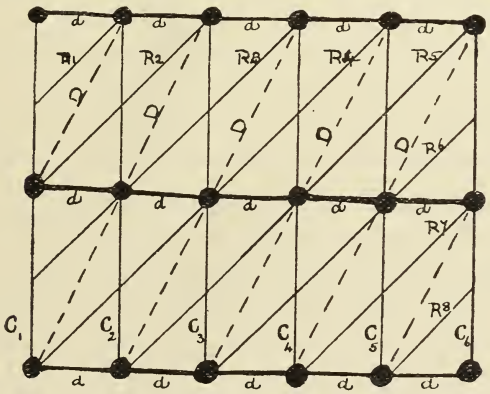

Fig. 8c.

Showing the cause of the development of the second set of ornament on *T. dædalea*.

is seen in the enlarged umbo in *Fig. 8b*. This accounts for the curious bent ornament in the upper umbonal region of *dædalea*.

About the fourth row down occurs an apparently second set of ornament, running about at right angles to the up-turned anterior limbs of the diagonals.

This, I believe, to be formed from the tubercles of the upper ends of lower diagonals. A possible explanation of the development of a second line may be seen on *Fig.* 8*c*.

Here the radials (R_1, R_2, &c.) cut the concentrics (C_1, C_2, &c.) at an angle of 45°. The tubercles seen on the figure are quincuncially arranged, as will be seen from the following chart:

	C_1	C_2	C_3	C_4	C_5	C_6
R_1	O	+				
R_2	+	O	+			
R_3	O	+	O	+		
R_4	+	O	+	O	+	
R_5		+	O	+	O	+
R_6			+	O	+	O
R_7				+	O	+
R_8					+	O

\+ represents a tubercle at the intersection of the concentric and radial.
O represents the absence of such a tubercle.
A blank indicates that the intersection is not visible on the diagram.

On the figure it will be seen that the diagonal distances D, even at so low an angle as 45°, are over twice as large as the distances *d* between the tubercles of succeeding diagonal rows. The result is that on the diagram the apparent lines of ornament run at right angles to the concentric. On the umbo of *T. dædalea* the crowding of the growth lines, and the high angle at which the radials and concentrics meet, accentuate this peculiarity and result in the obliteration of the true diagonal lines, and the seeming introduction of a second set of ornament.

On this working *T. dædalea* has the usual type of clavellate marking but it is highly modified by the peculiar form of the animal. There seems no need, therefore, to put it into a separate section, as has been done by Lycett. It certainly has no place in the *Undulatæ*, as suggested by

Etheridge,* as its umbonal markings are strictly diagonal. It seems to be merely a Cretaceous derivative of *T. clavellata.*

Other Types of Diagonal Ornament.

The *Scaphoideæ* [Lycett] are diagonally marked, but differ from the *Clavellatæ* in not having the ornament continued to where the flank bends round to face anteriorly at least. Consequently, the diagonals form straight lines (*Fig.* 6). The anteriorly-faced part of the flank is usually marked by transverse bars. These may possibly be due to the coalition of uplifted diagonals, as in the anterior parts of the diagonals of *T. clavellata*, in which case this group would be derived from the *Clavellata* proper by loss of the anterior part of the flank ornament.

In the Upper Lias of Robin Hood's Bay occurs *T. literata*, which has been placed by Lycett in the *Undulatæ.* This position is untenable, because its umbonal markings are diagonal.

Its flank markings, consist of well-marked diagonal rows running to about halfway across the flank In front of this are broken, more or less well-accentuated, growth lines which make a seeming V in the central part of the flank. On the anteriorly-faced part of the flank these may become better marked. This type of ornament seems to me to place it in the *Scaphoideæ* section of the diagonally ornamented *Trigoniæ.*

T. striata. This species is diagonally ornamented but differs from the others of this type in having spinous serrations instead of tubercles on the diagonal lines. A similar variation is seen in many of the *Scabræ.* What relationship, if any, there is between the two I do not know, but I hope to do further work on this.

* Etheridge, 1881. "On a new species of *Trigonia* from the Purbeck Beds of the Vale of Wardour." *Q. J. G. S.*, vol. 37, p. 246.

Manchester Memoirs, Vol. lv. (1911), *No.* **16.**

XVI. A Plesiosaurian Pectoral Girdle from the Lower Lias.

By D. M. S. Watson, M.Sc.

Read November 15th, 1910. *Received for publication January 24th, 1911.*

The Manchester Museum contains a series of Liassic reptiles, which were collected by the late Charles Moore, of Bath, but never incorporated in his collection; these reached the Museum through the good offices of Prof. Wm. Boyd Dawkins. The most important of them is a specimen of a small plesiosaur from the Lower Lias of the Bath district, most probably from Weston. This specimen shews uncrushed and slightly separated all the bones of the pectoral girdle, the left humerus and ulna, the right femur, and a long series of dorsal vertebræ. I at first referred it to *Plesiosaurus Hawkinsi*, a species to which the limb bones and vertebræ bear a very close resemblance. A restoration, made both on paper and with the aid of plasticene models, shewed, however, that the pectoral girdle was of a somewhat different type. I, therefore, made an attempt to develope one of the small fragments of cervical vertebræ which remained, and was able to expose the whole of the under surface, which, in the presence of a very marked hæmal ridge, differs from the corresponding bone of *P. Hawkinsi*. I am not yet able to make a definite identification of the species, but think it is probably *Plesiosaurus macrocephalus*.

This view is supported by an examination I made recently of the type specimen of *P. brachycephalus* in the

Bristol Museum, that specimen being, as Lyddeker has pointed out, merely an adult *P. macrocephalus.*

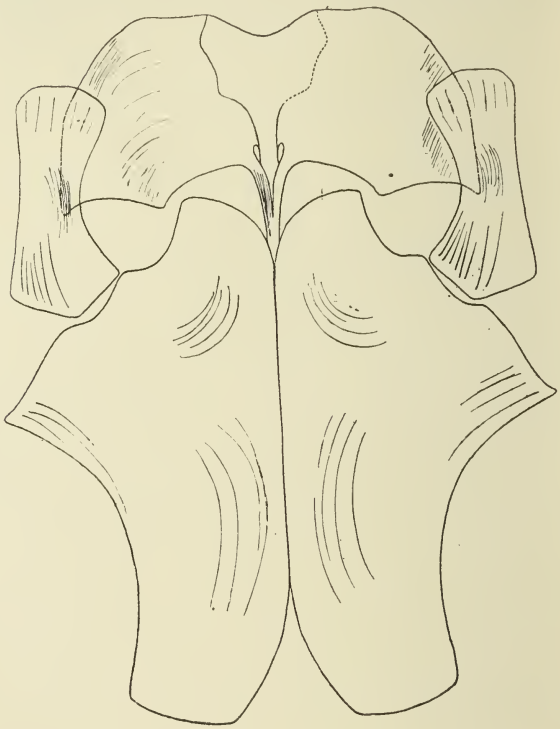

Fig. I. Restoration of the pectoral girdle of *Plesiosaurus macrocephalus.* Viewed from below × $\frac{1}{5}$.

The coracoids are not restored ; there is a perfect lower surface of the scapula and $\frac{3}{4}$ of the clavicular arch are preserved and are accurately represented in this figure. Notice especially the widely separated anterior ends of the scapulæ. L. 9767. Manchester Museum.

Description of the specimen.

Coracoid. Both coracoids are preserved, the right quite perfectly. They are exposed from their ventral surface.

The two coracoids meet in a long median symphysis, but separate from one another both anteriorly and posteriorly, leaving triangular areas, no doubt occupied by cartilage in life.

The measurements of the right coracoid are :—

Total length	20·3 cm.
Maximum breadth	10·8 cm.
Width of bone at posterior end ...	5·5 cm.
Length of preglenoid extension ...	5·5 cm.
Facet for glenoid cavity	4 cm.
„ (scapula)	3·3 cm.

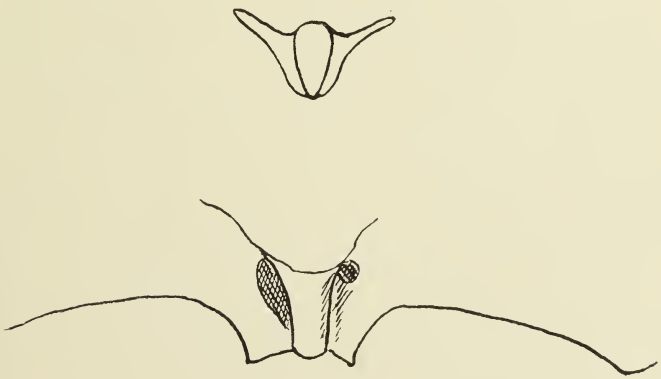

Fig. 2. The central and posterior part of the clavicular arch, × 1 to shew the relation of the interclavicular spine to the clavicules and interclavicular foramina.

The upper figure shews the section exposed on the fracture which terminates the spine, in natural position.

The bone is remarkable for the breadth and flattened form of the preglenoid extremity and its rather obscure demarcation from the articular facette for the scapula. The bone has the usual thickening between the glenoid cavity and the middle line. One striking character, which it shares with the majority of lower liassic plesiosaurs,

is the great narrowness of the posterior part of the coracoid, a condition as different as possible from that which obtains in such genera as *Cryptocleidus* and its allies.

The posterior part of the bone is quite thin and there is no trace of that thickened outer margin which is correlated with the postero-lateral process in such genera as *Cryptocleidus* and *Colymbosaurus*.

Scapulæ. Neither scapula is quite complete, but the right lacks only a small part of the inner side of the glenoid end, and has the end of the dorsal ramus still covered by matrix. The left scapula has a complete glenoid end, and perfectly supplements the right. The bone has the usual triradiate form. The lower surface is 9 cm. long, and is gently concave from back to front and from side to side. The glenoid end is slightly wider than the anterior end, both being considerably broader than the middle of the bone; as the exterior edge is sensibly straight, this implies the development of an inward extension passing under the clavicle.

The dorsal ramus is not exposed for its entire length, but as shewn, is very remarkable for its great breadth, which makes the entire bone very deep at the middle of its length. The dorsal ramus is directed almost exactly dorsally: its inner surface is convex across its breadth, a low, and somewhat obscure, ridge running down it, just behind its centre line. The external face of the scapula is very strongly concave, the outer border is a sharp edge from which the upper surface runs in leaving the bone, very thin until it is met by the thin ridge which is the continuation of the dorsal ramus.

The clavicular arch lacks a part of the left side and has the right lateral border obscured, but otherwise is excellently shewn. It appears to be quite uncrushed and

shews its natural form, a very rare occurrence. The middle part of the under side of the arch forms an essentially flat surface varied by a low median ridge which narrows and falls posteriorly into a spine. If this spine is placed horizontally the arch will slope downward in form, a feature of common occurrence in plesiosaurs. From this central comparatively flat region the lateral wings slope rapidly up at an angle of about 130 with the horizontal: at this bend the bone is about ·5 cm. thick. The anterior border is as a whole straight, but there is a central bay some 2·5 cm. wide and rather less than 1 cm. deep.

The posterior border of the arch has a median spine which is unfortunately broken off short, but is nearly 1 cm. thick. The bone on either side of this rod is very thin, and its border runs forwards and then turns gradually outwards and backwards until it slowly thickens and forms a strong process some 3·5 cm. from the middle line. Outside this process the border again runs slightly forward and outward until it reaches the outer edge of the bone. The exact shape of this posterior border is quite certain, part of it having been exposed by the splitting of the bone and part developed by myself.

It is not easy to follow the sutures separating the clavicles from the interclavicle in the anterior part of the arch, but posteriorly their relations are quite clear. The interclavicle runs backward as a spine of triangular section, the point of the triangle being downwards, and the clavicles pass backwards leaving a small foramen on each side of the interclavicular spine until they expand into flanges which are tightly attached to the sides of the interclavicular spine.

The mutual relations of the bones of the pectoral girdle are as follows :—The corocoids meet in the middle

line making at their symphysis a low obtuse ridge on the lower surface, the scapulæ are articulated with them so that their lower surface is practically in the same general plane as the coracoid of the same side, the clavicular arch has its lateral wings tightly adpressed to the inner surfaces of the scapulæ, the middle of the arch forms a rather broad plate, passing across between the widely-separated anterior ends of the scapulæ, and with its posterior border some distance in front of the anterior processes of the coracoids, except, possibly, for the median spine, which may have reached them. The anterior edge of the arch is directed slightly downwards, so that the arch, as a whole, is in a somewhat different plane to the coracoids.

The pectoral girdle just described is, on the whole, the most primitive known in the Plesiosauria: it shews that the primitive Sauropterygian had a T-shaped interclavicle, with narrow clavicles joining it on both sides. This T-shaped interclavicle seems to shew definitely that the median bone of the Sauropterygian clavicular arch is not a sternal element, for it is obviously similar to the T-shaped interclavicle of the majority of the early reptiles.

In correlation with the compression which the anterior border of the girdle has to bear in consequence of the forward thrust of the head of the humerus (see Andrews :10) the clavicular arch is strengthened by the backward growth of the clavicles along the posterior spine of the interclavicle, with which they are strongly united. This enlargement of the clavicles may also be associated with the development of a powerful musculus claviculo-brachialis inserted on to them and the lower and anterior part of the humerus. Such a muscle may have played an important part in the forward motion of the humerus, and have helped to turn it into position for the stroke. The two small foramina bounded by the clavicles and separated by the posterior

spine of the interclavicle can only imply the presence of a pair of small blood vessels, probably supplying muscles, passing up on each side of the interclavicle and persisting even after the backward growth of the clavicles had enclosed them; in any case they represent the first appearance of the interclavicular foramen, which is found in so many later plesiosaurs.

The whole girdle is of interest for its close resemblance to that of *Sthenarosaurus*, where also we find a T-shaped interclavicle, much more modified, however, than in this case, together with widely separated anterior rami of the the scapulæ, and a comparatively narrow clavicular arch. These two cases and *Plesiosaurus conybeari* are, in fact, the only ones in which the clavicles do not extend back on to the dorsal surface of the coracoids. The furcula of *Plesiosaurus arcuatus*, which is generally quoted as shewing a similar condition, is damaged, a similar bone in the British Museum (Natural History) and the conditions in the type specimen of the very closely allied *Plesiosaurus megacephalus*, shew that there was really a very delicate posterior extension of the clavicles meeting the coracoids.

XVII. The Upper Liassic Reptilia. Part 3. *Microcleidus macropterus* (Seeley) and the Limbs of *Microcleidus homalospondylus* (Owen).

By D. M. S. Watson, M.Sc.

Read November 15th, 1910. Received for publication January 24th, 1911.

The Sedgewick Museum at Cambridge only contains one Upper Liassic Plesiosaur, the beautiful skeleton which is the type of Seeley's *Plesiosaurus macropterus*. This specimen, which is wonderfully complete, has never been figured, and is so obscured by a very intractable matrix and black paint that little can be made of it.

Seeley gives the number of vertebræ as c. 39, d. 24, s. 1, cau. 28 ; he includes all the pectorals as dorsals. It is quite impossible to check these numbers with any pretence at accuracy, but they are not far from the truth, and indicate a reptile of the proportions of *Microcleidus homalospondylus*, which has c. 40, p.+d. 5+17, s. 3, cau. 20+.

None of the vertebræ are fit for description, but in all regions they appear to be of Microcleidus type.

Of the pectoral girdle only the dorsal rami of the scapulæ can be seen ; they project on each side of the pectoral region as narrow bones of oval section pointing slightly outward and backward, they agree exactly with the same parts of the scapulæ of *Microcleidus homalospondylus*. Their position shews that the whole girdle is present undisturbed, and that it is of the Microcleidus type, at any rate so far as the scapulæ go.

May 29th, 1911.

Of the pelvic girdle only the ilia are shewn ; they agree closely with those of the type species of *Microcleidus.*

The whole skeleton is in fact certainly that of a

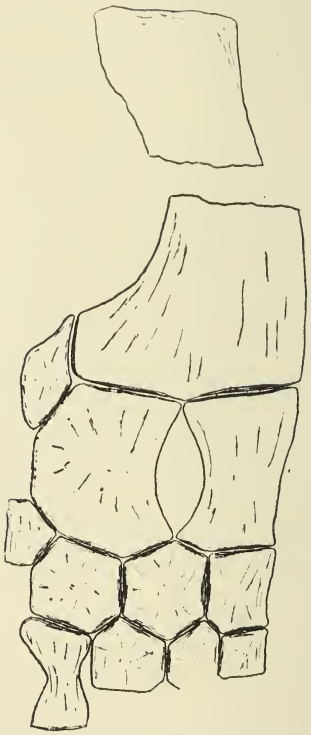

Fig. 1. Right fore paddle of *Microcleidus macropterus*, dorsal aspect. × $\frac{1}{6}$. Drawn from Seeley's type specimen in the Sedgewick Museum, Cambridge.

Microcleidus and the axial skeleton, and so much of the girdles as can be seen, present no definable differences to the type species of that genus, Owen's *Plesiosaurus homalospondylus.*

There are, however, certain slight differences of the limbs which suggest that the species is a good one. It is

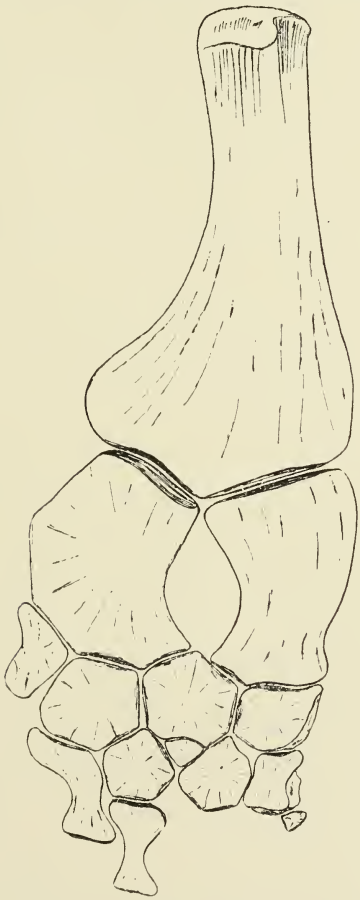

Fig. 2. Right fore paddle of *Microcleidus homalospondylus*, dorsal aspect. × ⅛.
L. 7077. Manchester Museum.

very probable that the specimen at York, which is one of Owen's co-types of *P. homalospondylus* really belongs to *M. macropterus* and not to the species to which it was originally referred. In this way the differences between

its pelvic girdle and that of the Manchester specimen, L. 7077, of *M. homalospondylus* may be accounted for.

All four limbs of the type specimen of *M. macropterus* are preserved, and although they are somewhat difficult to make out on account of the imperfect development of the specimen, do give an accurate idea of the structure of the Microcleidus limb.

For comparison with them there is only the fore limb of *M. homalospondylus* at Manchester, which is described later on in this paper.

The right limb of *M. macropterus*, which is the better preserved, is somewhat obscured by matrix, and has a fracture running across the middle of the shaft of the humerus.

The dimensions of the humerus are :—length 30 cms., breadth across the head 11 cms., across the distal end 18 cms., minimum width of shaft 9 cms.?

The head is obscured, the distal end of the bone has the usual flattened form. The anterior border is almost straight, the posterior is strongly concave. The distal end of the bone presents three facets for other bones. Those for the ulna and radius are straight, nearly in the same line, and approximately at right angles to the anterior border of the bone. The third is smaller, and is parallel to the anterior edge of the bone. It lies on the posterior edge.

These facets support the following three bones :—

1st. The radius, a flattened bone 9 cms. across proximally, 13 cms. long and 8 cms. wide distally. Its anterior border is nearly straight, but the posterior border is concave, so that the minimum width is only 6 cms.

2nd. The ulna, which is a flat bone of irregular shape, which will be best understood from *Fig.* 1.

3rd. A curious post-axial bone articulating with the humerus and the ulna. This corresponds with a bone figured by Owen in *Plesiosaurus rugosus* and by Andrews in *Tricleidus*.

The proximal row of the carpus is composed of four bones, preaxially, the radiale, a small pentagonal bone with facets for the radius, intermedium and carpalia 2 and 1.*

The intermedium is a hexagonal bone articulating with the radius and ulna, ulnare, carpalia 3 and 2, and the radiale.

The ulnare is also hexagonal, articulating with the ulna, pisiform, a small free space, the 5th metacarpal, by a small facet with the 3rd carpale, and with the intermedium.

The pisiform is a small pentagonal bone articulating with the ulna and the ulnare, and having three free faces which face proximally, postaxially and distally; the first and last are pitted for cartilaginous extensions.

The distal row of the carpus consists of the usual three bones.

This limb is remarkable for the very close and accurate fit of the bones composing it; they are all polygonal and not mere nodules as are the carpals of *Plesiosaurus* sensu strictu.

The post-axial bone lying between the humerus and the ulnare is curious, but is probably of no morphological importance. It may be due to the "supra-abundant bone-producing power" shewn by the very fully ossified carpus.

The hind limb is not sufficiently well exposed to be described: it appears to greatly resemble the fore limb,

* The numbers applied to the carpalia are throughout of purely descriptive significance.

but I could not determine the presence of the supernumerary post-axial bone.

The femur is rather more slender than the humerus. its anterior border is slightly concave.

The fore limb of *Microcleidus homalospondylus* is known from L. 7077 in the Manchester Museum, the specimen of which I have already described the girdles.

The humerus is 39 cms. long and 21 cms. across the distal end : the anterior border of the bone is straight, the posterior strongly concave, the distal end shews two facets, inclined to one another at an angle of about 135°, there is also a post-axial facet of small size and somewhat rounded. The head is set straight on the shaft, it is a rounded knob 8 cms. in diameter, pitted all over for a cartilaginous coat, with it is confluent the pitted surface of the great tuberosity, which has an area of 6 cms. by 4 cms.; this tuberosity gradually subsides on to the shaft.

The radius and ulna resemble exceedingly those of *M. macropterus*, they are of equal length, the post-axial supernumerary ossicle of the other species was probably originally present, although it is not preserved.

The radiale, intermedium, ulnare, and pisiform are exactly as in *M. macropterus*, except that the distal point of the intermedium is truncated and articulates with a small triangular bone completely surrounded by the intermedium, and the second and third distal carpalia ; this extra bone occurs in no other known Plesiosaur limb, and may be an individual abnormality ; in any case, it may represent one of the centralia which must have occurred in the ancestors of the Plesiosaurs.

Another small triangular bone, of which the exact position is unknown, is probably a pre-axial accessory bone similar to that figured by Fraas in the fore limb of

Plesiosaurus victor between the first metacarpal and the first distal carpal.

The most marked features of the limb are the extraordinary width of the carpus compared with the hand and the close fit of all the constituent bones.

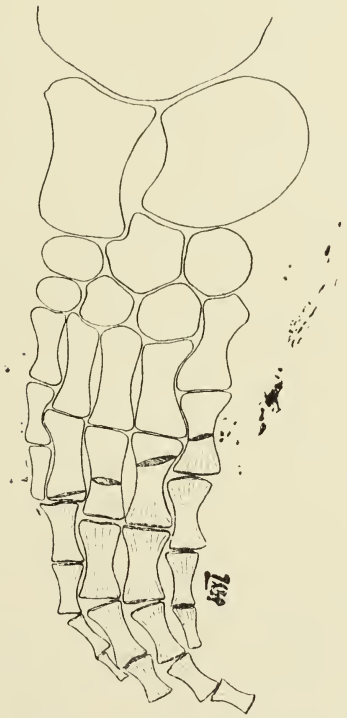

Fig. 3. Fore paddle of *Plesiosaurus macrocephalus*. $\times \frac{1}{3}$. L. 9766. Manchester Museum. To shew the apparent remains of the skin indicating the former existence of a posterior extension of the fin unsupported by bone.

This disproportionate width of the carpus raises the suspicion that the hand had in life a considerable posterior extension supported by non-calcified tissues. The existence of such an expansion will explain the great

massiveness of the fifth digit, and seems to be supported by a curious specimen at Manchester.

This specimen is a split slab of hard calcareous shale of light yellow colour, which contains the distal phalanges of a Plesiosaur limb, and the impression of the proximal portion, surrounding this limb in certain areas is a thin dark brown film, whose distribution is shewn by the black areas of *Fig.* 3 ; this film has a curious and distinctive appearance, and is, I think, certainly the preserved skin : on the anterior border it keeps quite near to the bone, but posteriorly it forms a large expansion which, I think, is probably natural, and represents the posterior extension, whose presence was so probable in *Microcleidus*.

A brown film, similar to that which surrounds this Plesiosaur paddle, also occurs in connection with some Icthyosaur skeletons at Manchester.

The hind limb of *Microcleidus homalospondylus* is solely represented by the femur, which is present in the Manchester specimen L. 7077.

The femur is a slender bone with concave anterior and posterior edges, its shaft is small and of oval section, the distal end shews two articular facets.

The bone is 36 cms. long and 19 cms. wide distally, the head is nearly hemispherical, being 8 cms. in diameter, and is confluent with the great trochanter, which is of very large size, its proximal termination forming a face 5 cms. square, inclined at 45° to the long axis of the bone. This face and the head of the bone are pitted, and in life must have been covered by the same cartilaginous coat.

The great trochanter gradually subsides into the shaft of the bone, which on its lower surface exhibits a triangular raised and roughened area, which marks an important muscle attachment.

The account of the limbs given above shews how slight is the difference separating *Microcleidus homalospondylus* and *M. macropterus*. It would be quite impossible to separate them on the evidence presented by any individual bone, but the fortunate occurrence of several good skeletons shews that they are certainly different.

The limbs shew an advance on the type most common in the Lower Lias in their much more extensive ossification, and also in the fact that the radius and ulna are of the same length ; in all the species from the Lower Lias the radius is very considerably longer than the ulna (*Fig.* 3).

Only one Upper Liassic species has yet been described which has limbs at all closely resembling those of our genus :—*Plesiosaurus Guilelmi Imperatoris*, of which the limbs strikingly resemble those of *Microcleidus* ; I believe the two types to be very closely connected, and shall discuss their relationship in a future paper when I describe the skull of *Microcleidus*.

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XVIII. Notes on some British Mesozoic Crocodiles

By D. M. S. Watson, M.Sc.

Read December 13th, 1910. Received for publication January 24th, 1911.

1. *Steneosaurus stephani*, Hulke.

The under surface of the skull was not described by Hulke in his description of this species ; it has recently been further developed, and now shews some interesting features.

The basi-occipital shews the usual single condyle for the atlas ; it is unusually short and flattened. The two lateral processes for muscular attachments are small in proportion to the great size of the skull ; they are separated by the pit in which lies the opening of the median eustachian tube. The grooves at the junction of the basi-occipital and basi-sphenoid, which lodged the canals leading from the lateral to the median opening of the eustachian tubes, are well marked.

The basi-sphenoid is only exposed on the ventral surface for a short antero-posterior space : it is as usual divided by a median ridge, with a wide flat surface, which separates the grooves leading down and round to the underside of the quadrate, the meaning of which is still obscure. *S. stephani* is unusual in that these grooves are entirely directed laterally.

Very little of the pterygoid remains. Only a small area some 4cms. long behind the posterior nares is represented by actual bone ; but the impression of the right pterygoid and ecto-pterygoid gives a good idea of what

appears to be the real outline of the bones. Their form will be best understood from *Figure* 1. The posterior nares instead of being rounded, as they usually are in typical members of the genus, are bounded anteriorly by the nearly straight posterior borders of the palatines. These form a bracket-shaped line with a notch in the centre.

The small scrap of pterygoid remaining shews a median ridge.

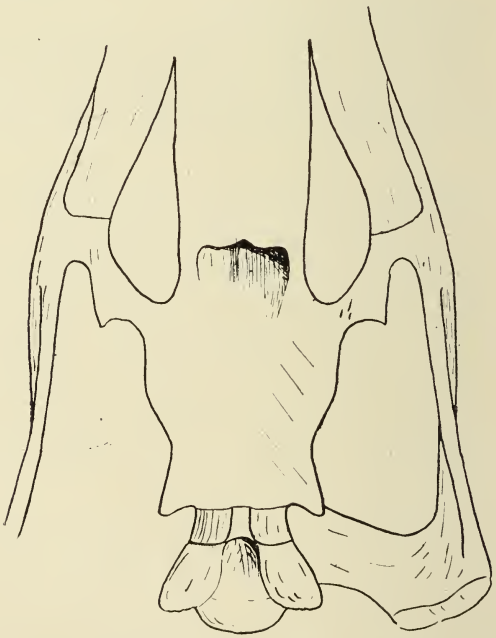

Fig. 1. *Steneosaurus stephani*, Hulke. Somewhat restored drawing of the under surface of the type skull. × $\frac{1}{4}$ to show especially the form of the posterior nares and the area of the pterygoids.

On the existing evidence it is impossible to determine whether there was any trace of the shallow pit or depression into which the posterior nares open in typical Steneosaurs and still more markedly in *Pelagosaurus*.

The surface of the palatines is generally flat, but there is a slight median ridge separating two shallow grooves.

The sub-orbital vacuities are short and broad and the ecto-pterygoids very feeble.

Steneosaurus stephani comes from the Cornbrash and is hence one of the earlier types of the genus. It is of interest to compare it with other early species.

In his "Notes paléontologiques" Eudes-Delongchamps figures a plaster cast of a skull from the English Cornbrash which he identifies with his *S. Boutilieri*. This skull consists almost entirely of rostrum and is hence rather difficult to compare with *S. stephani*: so far as corresponding portions of the two skulls occur, they agree, and it is probable that they belong to the same species. The Bristol Museum contains another copy of this cast, and I found there a cast of the back of a Steneosaur skull which may belong to it; it agrees closely with *S. stephani*, but as there is no real evidence that it belongs to the snout, I think it is preferable to keep the name *stephani* for the Closworth skull.

Compared with typical late members of the genus *Steneosaurus stephani* approximates to the *Mystriosaurus* type in its unusually large pterygoids and its relatively small supra-temporal fossae: from this genus, however, which I have studied in a magnificent series of specimens of the species *Brongniarti* at Manchester, *S. stephani* differs in the absence of pre-orbital vacuities, in not having the posterior narial opening pointed in front, and in the much smaller pterygoids.

In fact, in the condition of its pterygoids and supra-temporal fossae, it is exactly intermediate between *Mystriosaurus* and such a species as *Steneosaurus héberti*.

It is probable that the reduction of the pterygoids is to be correlated with the enlargement of the temporal muscles implied by the increase in size of the supra-temporal fossae, for this enlargement would imply a reduction of the pterygoidal muscles and hence of the pterygoid and ecto-pterygoid: the fact that this reduction increases regularly as time goes on shews that the Steneosaurs can have given rise to no eu-suchian form, for in all "procoelia" the pterygoidal muscles are greatly developed at the expense of the temporals.

2. *Metriorhynchus cp. hastifer: from the Corallian of Headington.*

Crocodilian remains other than isolated teeth are so rare in the Corallian that some account of a large part of the rostrum of a *Metriorhynchus* from the Corallian, Lower Calcareous Grit, of Quarry Field, Headington, may be of interest.

The specimen was obtained by Mr. Manning, and is now in the Manchester Museum, number L. 6459. It consists of a part of the snout including the tip of the frontal and terminated anteriorly by a transverse fracture in front of the nasals. So far as it goes it is quite perfect, being uncrushed. The skull was mutilated before fossilisation, the exposed edges being rounded. I do not consider that this indicates derivation of the specimen from some older bed but merely natural washing about on a beach.

The specimen is remarkable for its great solidity, the frontal being some 30mm. in thickness, and the pre-frontal of similar substance.

Viewed from above the specimen shews the maxillae meeting one another in the middle line for some 5cms. In this region they form an almost flat dorsal surface from

which the sides fall away leaving a somewhat marked edge. The nasals are large bones which meet in the middle line for 25cms. At the anterior end of the orbits

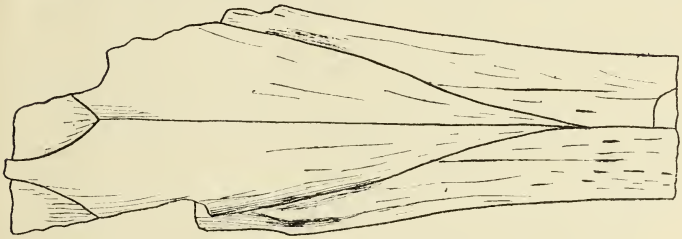

Fig. 2 (*a*).

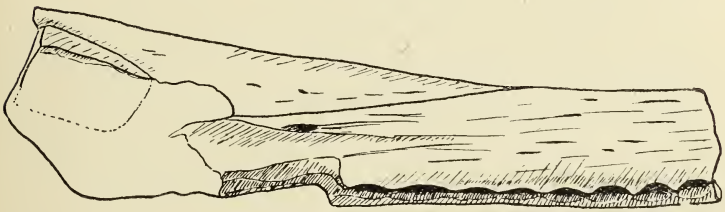

Fig. 2 (*b*).

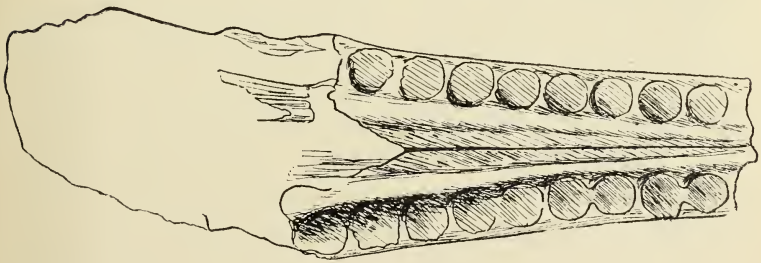

Fig. 2 (*c*).

Fig. 2. *Metriorhynchus cp. hastifer.* From the Corallian of Headington. Dorsal, right lateral and ventral views of the snout. ×¼. L. 6459. Manchester Museum.

where they are widest they measure 10·5cms. across. In the middle, and posteriorly they are very much swollen, so that the suture uniting them with the maxillae lies in a rectangular groove.

The frontal is only represented by a small anterior wedge, which lies between the posterior ends of the nasals. The naso-frontal suture is 6cms. long, probably about 8cms. when perfect. Only a small scrap of the right pre-frontal is preserved.

When the specimen is viewed laterally the dorsal surface presents a very gentle concave curve. At the anterior end the upper and lower surfaces become nearly parallel.

The chief feature shewn in the side view is a deep and very marked rounded groove on the surface of the maxilla and lachrymal. This groove leads into the orbit. It is present in all *Metriorhynchus* skulls that I have examined ; but as these are commonly preserved crushed flat in soft clay, it is not usually well shewn. As seen here it is a groove some 2cm. wide and 1cm. deep, leading in under the facet for the articulation of the pre-frontal. The lachrymal must be entirely covered by the groove. Leading out of this groove and plunging downwards and forwards into the cavity of the snout is a small foramen about 5mms. in diameter. This is situated in the suture between the nasals and the maxillae, and may conceivably represent the pre-lachrymal foramen of *Mystriosaurus*.

The large and well-marked groove represents a small smooth notch which occurs at the junction of the pre-frontal and lachrymal on the edge of the orbit in *Mystriosaurus* and *Pelagosaurus*, and in a much less marked form in *Crocodilus* and *Gavialis*. It probably transmits a small nerve to the facial region.

On the palatal aspect only the maxillae are preserved, posteriorly, they show depressed rough surfaces to which the palatines were attached. These latter bones extended forward to the level of the anterior end of the third alveolus. In front of this point the palate is divided into

three sub-equal divisions by rounded ridges running longitudinally; the outer grooves lodge the alveoli, of which 10 are visible on the right side, they are very large, averaging 2cms. in diameter. The central division is a deep channel, which is anteriorly divided by a slight rounded median ridge.

The specimen described above is obviously a *Metriorhynchus*, and in its great solidity and relatively broad snout is only comparable to three described species :—

Metriorhynchus brachyrhynchus, Deslongchamps.
M. *hastifer*, „
M. *palpebrosus*, Phillips.

Comparison with the figures and descriptions of *M. brachyrhynchus* given by Deslongchamps and Leeds and with the specimens in the British Museum and that at Caen, shew that the specimen under consideration cannot be referred to this species.

In *M. brachyrhynchus* the snout tapers more rapidly, the nasals are narrower anteriorly and broader posteriorly, the distance separating the nasals and pre-maxillae is smaller, and the palate is not so profoundly channelled as in our specimen. On the other hand, the number of teeth may have been similar.

The fragment appears to agree with *M. palpebrosus* in the gradual narrowing of the snout, and in the fact that the nasals never form the widest part in a transverse section of the skull. There are, however, 18 alveoli in *M. palpebrosus* in a space corresponding to 10 in our specimen, and the palate does not shew the very markedly channelled form which is so characteristic of our fragment.

M. hastifer differs from our fragment in the following characters :—

1st. The tapering of the snout is even slower than in our specimen.

2nd. The nasals are shorter.

3rd. In advance of the orbit the nasals form the widest part of a transverse section of the face.

On the other hand the palates agree exactly. Deslongschamps specially remarks on the pronounced medial channel of the palate.

On the whole the English specimen is probably best regarded as representing a well-marked variety of *S. hastifer*. I hesitate in applying a name to this variety on the evidence of the inadequate material before me.

It deserves to be noticed that Owen's *Steneosaurus temporalis* figured in the "British Fossil Reptiles," and said to be from the Bath freestones, is really founded on the type specimen of Phillips' "*Steneosaurus palpebrosus*" from the Kimmeridge clay of Shotover. Comparison of Owen and Phillips' figures will render this certain, Phillips' name has priority.

The fact that in this instance Owen has made an error in the geological age of a specimen throws doubt on the horizon of "*Steneosaurus geoffroyi* and *laticeps*" which are figured in the preceding plate as from the Great Oolite. Koken has already commented on their resemblance to *Macrorhynchus*, and they may quite easily be of later date. The present whereabouts of the specimens is unknown.

Owen's *Steneosaurus latifrons*, said to be from the Great Oolite of Northamptonshire, is undoubtedly founded on the specimen in the Sharp collection from the Upper Lias. Comparison of Owen's figure with the specimen renders this certain, for some areas which are restored in plaster on the specimen are indicated as being absent

in the figure. It is difficult, allowing for the different class of preservation, to see how *Steneosaurus latifrons*, Owen, differs from *Steneosaurus brevior*, Blake. The species is really a *Mystriosaurus*.

3. *Metriorhynchus hastifer from the Kimmeridge clay of England.*

The Sedgewick Museum at Cambridge contains the anterior end of the snout of a *Metriorhynchus* from the Kimmeridge clay of Ely that undoubtedly belongs to *M. hastifer*. The specimen is of interest, because it shews the very perfectly preserved anterior nares, which are of the ordinary type. The tip of a tooth is visible in one of the alveoli. It has a crown covered with fine irregular, more or less longitudinal, wavy ribs. It appears to be definitely identical with the tooth figured by Phillips as *Steneosaurus longirostris*, Cuv. Phillips' specimen consists of the alveolar borders of a large part of the upper jaw, and so far as it goes appears to agree closely with the type specimen of *M. hastifer* now at Paris.

The Oxford University Museum also contains a very characteristic frontal of this species from the Kimmeridge clay of Shotover.

4. "*Petrosuchus laevidens*" *from the Purbeck of Swanage.*

This species was founded by Owen on a skull and mandible from the Middle Purbeck of Swanage, theoretically associated together.

The skull is exceedingly crushed and somewhat weathered, but with care all the sutures of the upper surface can be made out. They are shewn in *Text-figure* 3. When this figure is compared with the figure of *Macrorhynchus schaumbergensis*, given by Koken, no

differences of generic value are to be seen, and the only character recorded by Owen and Lyddeker which would serve to separate it from that genus is concerned with the position of the posterior nares. In *Macrorhynchus* these are placed very far back—nearly as in a modern gharhial ; whereas in *Petrosuchus* they are said to be in the middle of the skull and to be flanked by a pair of projections. Actual examination of the specimen shews that it is far too badly preserved to justify any such contention ; the

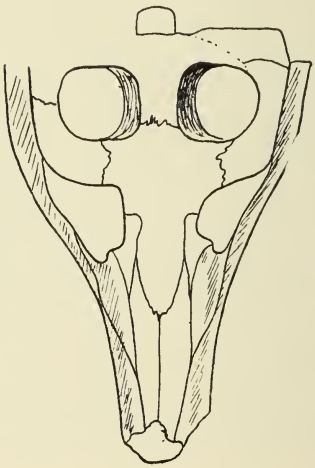

Fig. 3. Dorsal surface of the type skull of *Pholidosaurus decipiens (olim Petrosuchus laevidens).* $\times \frac{1}{4}$ to show the sutures between the various bones which are only partly figured by Owen.

lower surface is crushed, and it is impossible to distinguish between the matrix and the bone, to which it very tightly adheres. The two projections, which have been supposed to mark the position of the anterior edge of the posterior nares, are really formed by the crushing through of the descending processes of the pre-frontals, which articulate with the anterior ends of the pterygoids.

Their position favours this suggestion, and they can be exactly paralleled in crushed skulls of *Mystriosaurus*.

There is hence no reason for separating the skull described as *Petrosuchus laevidens* from *Macrorhynchus*, which Koken holds to be synonymous with *Pholidosaurus*, H. v Meyer, which has priority.

All the skulls of *Pholidosaurus* have a very elongated rostrum, being of gavialoid proportions. The *Petrosuchus* skull is damaged anteriorly, so that no direct evidence of its length is possible.

The lower jaw, which Owen theoretically associated with the skull, is quite short, and seems to shew that the

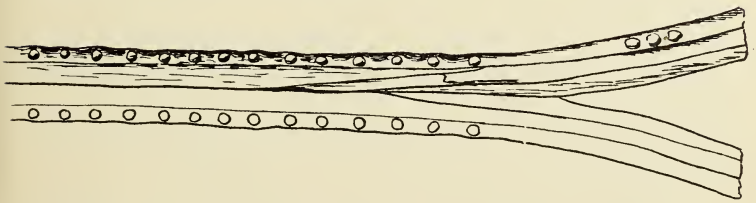

Fig. 4. Lower jaw of *Pholidosaurus ? decipiens*, from the Middle Purbeck of Swanage. $\times \frac{1}{3}$.

symphysis was short, only some 2 or 3cms. in length. This jaw, in the posterior part not figured by Owen, has an ornament of deep rounded pits like those on a Goniopholis scute. The skull shews only a very faint ornament of irregular shallow grooves, even the frontal not bearing any deep pits.

It is, in fact, certain that the skull and lower jaw described as *Petrosuchus laevidens* have nothing whatever to do with one another. There is in the Manchester Museum a portion of the right ramus of the lower jaw of a crocodilian from the Middle Purbeck of Swanage which agrees closely with that figured by Koken for *Pholidosaurus schaumbergensis*. The ornament of this

jaw is similar to that of the *Petrosuchus* skull, and it appears to correspond in curve. The length preserved is 29cms., of which 27 are tooth-bearing. Of this length 17cms. are included in the symphysis. The splenial comes well into the symphysis. The dental alveoli are small and well separated, the bone between them being roughened. A transverse section across the ramus in the symphysial region is nearly a quadrant of a circle.

The discussion just concluded leads to a curious nomenclatural difficulty, which is as follows :—

Owen described the genus and species *Petrosuchus laevidens* on a skull and lower jaw which do not belong to one another; the skull belongs to v. Meyer's genus, *Pholidosaurus*, and as it is described earlier in the text, should strictly be taken as the type of the species, which would then be known as *Pholidosaurus laevidens* (Owen). This name would be misleading, because the skull shews no teeth, and even the alveoli are not exposed.

On the whole it seems best to retain the name *Petrosuchus laevidens* for the mandible, which does not accurately correspond with any known genus and species, and give a new specific name to the skull, under these circumstances I propose that the skull should be known in future as *Pholidosaurus decipiens*, Watson.

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Manchester Memoirs, Vol. lv. (1911), *No.* **19.**

XIX. The Development of the Atomic Theory:—(6) The Reception accorded to the Theory advocated by Dalton.

By Andrew Norman Meldrum, D.Sc.

(Communicated by Mr. R. L. Taylor, F.C.S., F.I.C.)

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"From the nature of the human mind, time is necessary for the full comprehension and perfection of great ideas." Thus the history of an idea necessarily includes the reception accorded to it on publication, and the steps by which it came to be of influence in the world.

Science, considered as an impersonal thing, advances by assimilating new and sound ideas. Yet this process of advancement, as the following paper shows, depends on the temperaments of individual men. The consideration of paramount importance in this respect is the fact that these men, according to their outlook on matters of theory, can be divided into two classes. There are (1) the men who are alive to the immense value of theory in science, and (2) the men who would confine science to a collection of facts and laws, as if it were "based entirely upon experiment or mathematical deductions from experiment."[1]

At any given time, the direction in which a particular branch of science advances is determined by a few persons only. Consequently, the men who are inimical

[1] P. G. Tait, "Recent Advances in Physical Science," p. 10.

to theory may exert a harmful effect on science, by despising and rejecting a theory of the utmost importance.

The usefulness to science of the atomic theory is so completely established now, that it must seem strange to us to observe the efforts Dalton had to make, in order to arouse attention to the importance of his ideas regarding atoms. For some nine years, (1801-1810), if not longer, he endeavoured to spread abroad his ideas, both by private communications and publicly, by his writings and by lectures in various parts of the country.

As will be seen, Dalton's speculations had to encounter dangers of two kinds. In the first place, not many people gave themselves much concern about the question of the continuity or discontinuity of matter. They were quite content to go on speaking of "atoms" and "molecules" in a vague, colloquial sense, and Dalton had to induce them, if possible, to use the words as terms of precision. This done, there was always the possibility that they would reject Dalton's idea of an atom as too hypothetical.

His physical atomic theory (described in the fourth paper of this series) was devised in the year 1801, from which time onwards he made many attempts to recommend it to the scientific world. But for years the only avowed adherent which it obtained was William Henry. The question at issue was a fundamental one, and Dalton's finding on it was ultimately triumphant. The theory expressed his conviction that the diffusion of gases is due to physical forces and not to chemical. But the prevailing tendency of the time was to regard diffusion as due to chemical affinity between the gases concerned, and the strength of that tendency was exhibited by the amount of opposition to Dalton's theory. Its opponents included

Thomas Thomson, John Murray, T. C. Hope, John Gough, Humphry Davy, and Claude Louis Berthollet.

Dalton's chemical theory was formed by the 6th September, 1803,[2] and he proceeded forthwith to extend and apply it, and make it known in every direction. His first efforts, naturally, were made at this Society, where, on the 7th October, he read a paper in which the theory was employed in order to explain the absorption of a gas by water. What he endeavoured to do was to establish a connection between the solubility of a gas and its atomic weight. This paper, as published in 1805, comes to an end with a table of atomic weights, of various simple and compound substances, remarkable as the earliest of the kind ever printed. There is no reason to doubt that the paper contained a table of atomic weights when it was read, but Dalton certainly extended the table before going to press.

Dalton was eager both to have his ideas put into circulation and to have a resumé of them put on record. In London, in the winter of 1803-1804, he gave a course of lectures at the Royal Institution, in which he included a brief outline of the theory. He left this for publication in the Journals of the Institution, but, as he ironically remarked afterwards, "he was not informed whether that was done."[3] Humphry Davy was at the Institution at the time, but Dalton did not succeed in arousing in him

[2] A paper of his, read before this Society on November 12th, 1802, contains a reference to the chemical theory. This is the paper "on the proportion of the several gases, or elastic fluids, constituting the atmosphere." But it was not published till 1805, and Roscoe and Harden think it was re-written in the meantime, for it includes results, on the combination of nitric oxide and oxygen, which Dalton did not obtain till 4th August, 1803. (Roscoe and Harden, "New View of the Origin of Dalton's Atomic Theory," p. 35). I cannot myself doubt that this conclusion is the correct one.

[3] "New System of Chemical Philosophy," 1808, Preface.

any interest in the theory, much less any enthusiasm for it. In a course of lectures which he gave in Manchester in 1805, he included an account of the theory.[4] But it was not taken up there for years, not even by William Henry. There is not the slightest sign that Dalton would not have welcomed workers on the subject. But the atoms were counted an airy or recondite speculation. Dalton's atomic weight data caused no thrill of excitement, aroused no eager curiosity, no consuming wish to join in his work.

In North Britain Dalton had a different reception. Early in the year 1807 he gave a course of lectures, twice in Edinburgh and once in Glasgow. In Edinburgh he says "a class of eighty appeared for me in a few days." At the conclusion "several of the gentlemen who had attended the course represented to me that many had been disappointed in not having been informed in time of my intention to deliver a course, and that a number of those who had attended a first course would be disposed to attend a second."[5] This reception afforded Dalton precisely the encouragement of which he stood in need. "On these occasions," he said, "he was honoured with the attention of gentlemen, universally acknowledged to be of the first respectability for their scientific attainments: most of them were pleased to express their desire to see the publication of the doctrine in its present form, as soon as convenient. Upon the author's return to Manchester he began to prepare for the press."[6]

[4] Two unpublished papers, read before the Manchester Society in 1804, probably included accounts of the theory. The titles are respectively "A Review and Illustration of some Principles in Mr. Dalton's course of lectures on Natural Philosophy at the Royal Institution in January, 1804," and, "On the Elements of Chemical Philosophy."

[5] Angus Smith, "Memoir of Dalton," p. 58.

[6] "New System of Chemical Philosophy," 1808, Preface.

In the "New System of Chemical Philosophy" both the Preface and the Dedication show that Dalton was immensely grateful for the attention his speculations received in Glasgow and Edinburgh. The dedication runs:—"To the Professors of the Universities and other residents of Edinburgh and Glasgow who gave their attention and encouragement to the Lectures on Heat and Chemical Elements, delivered in those cities in 1807: and to the members of the Literary and Philosophical Society of Manchester, who have uniformly promoted his researches."

An account of the theory, often referred to in this series of papers, had already appeared. Thomas Thomson had been so much interested and impressed by the doctrine as Dalton explained it to him in 1804, that he became the first convert to it. He showed as much zeal in the cause as its author. With permission he gave a short sketch of it in the next edition of his "System of Chemistry." This was the third edition, published in 1807, of the most successful treatise of the day on chemistry, and it had more influence, directly, in spreading a knowledge of the doctrine than Dalton's own efforts. It made Dalton's theory known to William Hyde Wollaston in London, to Claude Louis Berthollet in France, and to Amadeo Avogadro in Italy.

Thomson found another opportunity of expounding the theory in his memoir "On oxalic acid," which appeared in the *Philosophical Transactions* of the Royal Society of London in 1808. The very next paper in the *Transactions* is one by Wollaston—on the carbonates and oxalates of potassium—and he, as well as Thomson, advanced his work as exemplifying and justifying Dalton's theory. By these various means, Dalton's and Thomson's books,

and Thomson's and Wollaston's memoirs, it became known in Britain and France, in Italy and Sweden.

Not only in these ways, but by personal exertions, Thomson and Wollaston sought to advance the theory. In his "History of Chemistry," Thomson gives a narrative of the efforts that had to be made to induce Humphry Davy to take it seriously. Long as the narrative is, it is quoted here almost in full, for it illustrates the fact that in science the spread of new ideas depends as much on personal efforts, springing from genuine conviction, as on printed papers. It would seem that Thomson and Wollaston failed themselves to persuade Davy. Wollaston, however, converted Davies Gilbert, and he, in his turn, succeeded in converting Davy.

"Some of our most eminent chemists," says Thomson, "were very hostile to the atomic theory. The most conspicuous of these was Sir Humphry Davy. In the autumn of 1807 I had a long conversation with him at the Royal Institution, but could not convince him that there was any truth in the hypothesis. A few days after I dined with him at the Royal Society Club, at the Crown and Anchor in the Strand. Dr. Wollaston was present at the dinner. After dinner every member of the club left the tavern, except Dr. Wollaston, Mr. Davy, and myself, who staid behind and had tea. We sat about an hour and a half together, and our whole conversation was about the atomic theory. Dr. Wollaston was a convert as well as myself; and we tried to convince Davy of the inaccuracy of his opinions, but, so far from being convinced, he went away, if possible, more prejudiced against it than ever. Soon after, Davy met Mr. Davis [*sic*] Gilbert, the late distinguished president of the Royal Society, and he amused himself with a caricature description of the atomic theory, which he exhibited in so ridiculous a light, that Mr. Gilbert

was astonished how any man of sense could be taken in with such a tissue of absurdities. Mr. Gilbert called on Dr. Wollaston (probably to discover what could have induced a man of Dr. Wollaston's sagacity and caution to adopt such opinions), and was not sparing in laying the absurdities of the theory, such as they had been represented to him by Davy, in the broadest point of view.

"Dr. Wollaston begged Mr. Gilbert to sit down, and listen to a few facts which he would state to him. He then went over all the principal facts at that time known respecting the salts; mentioned the alkaline carbonates and bicarbonates, the oxalate, binoxalate, and quadroxalate of potash, carbonic oxide and carbonic acid, olefiant gas and carburetted hydrogen; and doubtless many other similar compounds, in which the proportion of one of the constituents increases in a regular ratio. Mr. Gilbert went away a convert to the truth of the atomic theory; and he had the merit of convincing Davy that his former opinions on the subject were wrong."[7]

Thomson goes on to say that Davy "ever after was a strenuous supporter" of the atomic theory. This puts his support of the theory far beyond its true value. Davy was never enthusiastic about the doctrine of atoms as such, and he much preferred the term "proportion" to "atom." The following passage, published in 1811, probably represents his mature opinion on the subject:— "it is not, I conceive, on any speculations upon the ultimate particles of matter, that the true theory of ultimate proportions must ultimately rest."[8]

Dalton himself was far from satisfied with the reception accorded to his theory. Hope, of Edinburgh,

[7] Thomson, "History of Chemistry," vol. 2, p. 293.

[8] *Phil. Trans.*, 1811, Bakerian Lecture, or Davy's Works, vol. 5, p. 326.

could not bring himself to accept it.[9] It was criticised adversely by Dr. Bostock in *Nicholson's Journal*, and Dalton, in reply, quoted in support of it analyses by Dr. Bostock. He then remarks :—"A number of such analyses as these would compel Dr. Bostock and others of your chemical readers to examine the theory of chemical combinations which I have offered to them with more attention, than I fear they do. The present state of chemical science imperiously demands it,"[10]

In France, also, the theory was coldly received. Berthollet naturally opposed it, for in its general tendency it condemned his attempt to obliterate the distinction between physical and chemical forces, and, in particular, it was contradictory of his doctrine of chemical combination in indefinite proportions (see the first paper of this series). He considered Dalton's theory too hypothetical, and his opposition had great influence. Gay-Lussac, who had been his pupil, was unable to "rid himself of preconceptions due to early training." In his famous memoir, on the proportions by volume in which gases combine, he remained an adherent of Berthollet.

Gay-Lussac was always timid in matters of theory. Such was his temperament. On one occasion he laid it down that "in natural science, and, above all, in chemistry, generalisation should come after, and not before, a minute knowledge of each fact."[11] Such a man was not very likely to subscribe to a doctrine like Dalton's, which *promised* to transform the whole province of chemistry. Gay-Lussac admitted the *facts* adduced by Dalton and Thomson and Wollaston, and that was all.

Gay-Lussac conceded to Dalton as much as he must, and nothing more. From his own results it seems obvious

[9] Roscoe and Harden, *op. cit.* p. 153.
[10] *Nicholson's Journal*, vol 29, p. 150, 1811.
[11] *Ann. Chim. Phys.*, vol. 11, p, 297, 1819.

now that there must be very simple ratios between the volumes occupied by different atoms in the gaseous state. Many writers[12] have assumed that Gay-Lussac, in his memoir, defined the relation between his law and the atomic theory, but, as a matter of fact, he ignored it. He did not recognise the theory, and the subject was neglected and came to nothing in France for years. At length, in 1814, Ampère published a memoir,[13] of which the fundamental idea is that the molecules of different gases under the same conditions have the same size, so that equal volumes of different gases contain the same number of molecules. In this memoir, the first outcome of the modern atomic theory in France, Dalton is not mentioned.

In Italy Amadeo Avogadro had forestalled Ampère by three years.[14] Under the stimulus of Dalton's speculations, of which he had learnt through Thomson's "System of Chemistry,"[15] he composed the memoir in which he advanced and maintained his famous hypothesis, that under the same conditions the molecules of different gases occupy the same volume. This hypothesis, involving as it did a distinct departure from Dalton's ideas, became the fundamental dogma of molecular science only after the lapse of fifty years.

In Sweden J. J. Berzelius had been occupied for some time in determining the composition of metallic salts, when Wollaston's memoir reached him.[16] Forthwith he set

[12] See, for instance, Clerk Maxwell, "Theory of Heat," 10th ed., p. 326.

[13] *Ann. Chim.*, vol. 90, pp. 43-86, 1814.

[14] *Jour. Phys.*, vol. 73, pp. 58-76, 1811.

[15] "In what follows, I shall make use of the exposition of Dalton's ideas which Thomson has given us in his 'System of Chemistry,'" *loc. cit.*, p. 62.

[16] *Phil. Mag.*, vol. 41, p. 3, 1813.

himself the task of testing the validity of Dalton's theory on the grand scale. As he said, "this way of regarding chemical compounds at once throws such a clear light on the doctrine of affinity, that if the hypothesis of Dalton could be proved, it should count as the greatest step that chemistry had made towards its perfection as a science."[17]

As Lord Morley has pointed out, the people who launch great ideas on the world are seldom the people who apply them. Dalton's and Thomson's efforts to make the atomic theory widely known were really far more valuable than the concrete results they obtained by the use of it. Dalton himself was involved by the theory in a "labyrinth of chemical investigation," where he wandered for many years and wasted his energies. It was Berzelius, and no other, who applied it, made it the foundation of accurate chemical analysis, and proved it to be an organon of incomparable power for the advancement of chemistry.

[17] *Loc. cit.*

XX. The Conditions that the Stresses in a Heavy Body should be purely Elastic Stresses.

By R. F. Gwyther, M.A.

Read and received March 7th, 1911.

Analytical Preface.

To avoid breaking the continuity of the argument in the main body of the paper, I prepare for a change from the usual notation by introducing in this preface a modification which I propose to introduce into the treatment of the elastic equations for a spherical shell as usually given in text-books.

The Elastic Equations for a Spherical Shell.

The displacements in the directions r, θ, ϕ are generally written u, $r\beta$ and $r \sin\theta\, \gamma$. I propose, in the first place, to omit the factor r in the last two cases, and to introduce the factor $\sin\theta$ in the second case.

By this means $\sin\theta$ will also become an obvious factor in other cases, and the remaining functions will be functions of $\cos\theta$. Following precedent, I shall write x for $\cos\theta$, and for convenience I shall introduce a differential coefficient with regard to x in the second of the displacements.

Shortly, I shall write the displacements as

$$u,\ \sin\theta \frac{\partial v}{\partial x},\ \sin\theta\, w.$$

May 22nd, 1911.

Then, we shall have for the elements of the strain,

$$e=\frac{\delta u}{\delta r},$$

$$f=\frac{1}{r}\left(u+x\frac{\delta v}{\delta x}-(1-x^2)\frac{\delta^2 v}{\delta x^2}\right),$$

$$g=\frac{1}{r}\left(u+x\frac{\delta v}{\delta x}+\frac{\delta w}{\delta \phi}\right);$$

$$a=\frac{1}{r}\left(\frac{\delta^2 v}{\delta x\delta \phi}-(1-x^2)\frac{\delta w}{\delta x}\right),$$

$$b=\frac{\sin\theta}{r}\left(\frac{1}{1-x^2}\frac{\delta u}{\delta \phi}+r\frac{\delta w}{\delta r}-w\right),$$

$$c=\frac{\sin\theta}{r}\left(r\frac{\delta^2 v}{\delta r\delta x}-\frac{\delta v}{\delta x}-\frac{\delta u}{\delta x}\right),$$

and

$$\Delta=\frac{\delta u}{\delta r}+\frac{1}{r}\left(2u+2x\frac{\delta v}{\delta x}-(1-x^2)\frac{\delta^2 v}{\delta x^2}+\frac{\delta w}{\delta \phi}\right).$$

And, for the rotations,

$$2\omega_1=\frac{1}{r}\left(2xw-(1-x^2)\frac{\delta w}{\delta x}-\frac{\delta^2 v}{\delta x\delta \phi}\right),$$

$$2\omega_2=\frac{\sin\theta}{r}\left(\frac{1}{1-x^2}\frac{\delta u}{\delta \phi}-r\frac{\delta w}{\delta r}-w\right)=2\sin\theta\,.\,\Theta_2.$$

$$2\omega_3=\frac{\sin\theta}{r}\left(r\frac{\delta^2 v}{\delta r\delta x}+\frac{\delta v}{\delta x}+\frac{\delta u}{\delta x}\right)=2\sin\theta\,.\,\Theta_3,$$

whence

$$\frac{\delta}{\delta r}(r^2\omega_1)-\frac{\delta}{\delta x}(r(1-x^2)\Theta_2)+\frac{\delta}{\delta \phi}(r\Theta_3)=0.$$

The general equations of elastic motion under gravity become

$$(m+n)\frac{\delta\Delta}{\delta r}+\frac{2n}{r}\left\{\frac{\delta}{\delta x}((1-x^2)\Theta_3)+\frac{\delta\Theta_2}{\delta \phi}\right\}=\rho(\ddot{u}+gx),$$

$$(m+n)\frac{\delta\Delta}{\delta x}+2n\left\{\frac{1}{1-x^2}\frac{\delta\omega_1}{\delta \phi}-\frac{\delta}{\delta r}(r\Theta_3)\right\}=\rho r\left(g-\frac{\delta\ddot{v}}{\delta x}\right),$$

$$(m+n)\frac{\delta\Delta}{\delta \phi}-2n\left\{\frac{\delta}{\delta r}(r\Theta_2)+\frac{\delta\omega_1}{\delta x}\right\}=\rho r\ddot{w}.$$

Limiting ourselves now to the case of equilibrium under circumstances of symmetry about the vertical axis, the equations reduce to

$$(m+n)\frac{\partial \Delta}{\partial r} + \frac{2n}{r}\frac{\partial}{\partial x}((1-x^2)\Theta_3) = g\rho x,$$

$$(m+n)\frac{\partial \Delta}{\partial x} - 2n\frac{\partial}{\partial r}(r\Theta_3) = g\rho r,$$

$$\frac{\partial}{\partial r}(r\Theta_2) + \frac{\partial \omega_1}{\partial x} = 0.$$

The last equation becomes

$$r\frac{\partial^2}{\partial r^2}(r\overline{\omega}) + \frac{\partial^2}{\partial x^2}((1-x^2)\overline{\omega}) = 0,$$

and need not be carried further.

The solution of the first two equations is given by

$$(m+n)\Delta = g\rho rx + 2n\frac{\partial}{\partial r}\left(u + \frac{\partial(vr)}{\partial r}\right),$$

with

$$u + \frac{\partial(vr)}{\partial r} = \Sigma\left(A_p r^p + \frac{B_p}{r^{p+1}}\right)V_p,$$

where V stands for a zonal harmonic.

We can complete the solution by taking

$$u = \Sigma u_p V_p \text{ and } v = \Sigma v_p V_p.$$

In accordance with the notation introduced in the displacements, it will be convenient to write the elements of the stress P, Q, R, S, $T\sin\theta$, and $U\sin\theta$, so that P, Q, R, S, T, and U are functions of $\cos\theta$ or x.

General Outlines of Argument regarding Stresses and Elastic Stresses.

If a heavy body, say a cylinder, stands on a horizontal plane in equilibrium, and is then held in the same orientation by a grip on its upper surface, the supporting plane being

removed, we deduce from statical considerations that the alteration of vertical stress across every horizontal section is equal to the weight of the body. The consequences may be that rupture takes place, or that the strains exceed the elastic limit, but if there is justification for believing that the strains are small and within the elastic limit, we proceed to equate the stresses to the corresponding functions of the strain and obtain the displacements in the body.

If, again, we consider a massive structure built up *in situ*, the cases may vary greatly from, say, the substance of the Earth to the greater or less structures raised by human hands. The materials may be complex and of very various characteristics. For the purpose of my argument I shall imagine a concrete wall built in courses of equal heights and so constructed as to allow us to assume a complete bond between the courses.

The material of each course is initially in a fairly liquid state able to run under hydrostatic pressure. As it sets it develops in some way a capacity to exert other stresses than hydrostatic pressure and finally becomes a solid mass.

As the wall is built up additional stresses are caused in each course by the weight of the superincumbent courses. Finally, we have a wall in which the stresses vary periodically (in the Fourier sense). There appears no justification for claiming that the stresses are elastic stresses, or have the character of such stresses. If, however, we could remove the original structural stress in each course from the whole system of stresses, we might fairly assume the character of elastic stresses for the remaining portion of the stress system.

Mathematically, this process now suggested is not greatly different from that employed in the first simple

example. In each case we should deal with the difference of two stress systems. The practical difference is that we cannot handle the massive structure.

In the case of the substance of the Earth, there can be no justification for assuming the whole stress system to be elastic, although a part of it may be so treated.

General Principles.

In text-books on Elasticity and in numerous investigations on that subject, elastic equilibrium "under bodily forces" is treated in a method far from satisfactory, except as descriptive of displacements consistent with elastic stress. The method is to assume that the whole stress system is elastic, and to form equations which can to some extent be solved, and to state that difficulty arises in satisfying the surface-conditions. This method hides what may be the real question at issue, and throws upon the surface-conditions difficulties which may have been introduced by the initial hypothesis, because the stress system is not an elastic stress system.

I write this paper to propose a different order of treatment, in which the solution of the elastic equations may be convenient and desirable, but will not be the prime essential. It will be necessary to show by examples that the method proposed is a feasible one.

The method is first to solve formally the statical stress equations "under bodily forces" for a body of the shape under consideration. There will be three equations connecting the six elements of the stress, and the solution will give the six elements of the stress in terms of the force system and three quantities conditioned by the form of the bounding surfaces. The stresses so found must satisfy such structural conditions as may apply to the case: the

body may yield but it must not collapse or visibly suffer change of shape. A portion of the solution will indicate definite gravitational structural stresses, and a further portion will indicate 'complementary' stresses, conditioned but not determined.

Then, as an assumption, these stresses may be equated to the corresponding expressions in terms of strain for the purpose of determining the conditions under which the hypothesis is admissible, if admissible at all, and finally of determining the displacements when the stress is really elastic.

If we eliminate the conditioned functions we should obtain the elastic displacement equations. This may be convenient, but is not necessary. The conditions which it is proposed to find are to be obtained by eliminating the components of the displacement in every possible way, or if it is found more convenient in any case, the displacement equations may be solved first, provided it is recognised that the conditions of possibility still require to be investigated.

If a body is rectangular, the difficulty of finding a formal solution of the statical equation will be great, but this case offers the best opportunity of estimating the number of conditions.

The elements of a stress, generally, are subject to three statical conditions. If the stress is a purely elastic stress its elements are subject to six further differential conditions. Hence the assumption that the stress in a heavy body is a purely elastic stress involves a considerable assumption, but allows of estimation by known formulæ.

Stresses in a Spherical Shell.

Taking the polar system of co-ordinates (r, θ, ϕ), I shall write the elements of the stress $(P, Q, R, S, T\sin\theta, U\sin\theta)$,

and in the resulting equations I shall replace $\cos\theta$ by x. I shall also assume symmetry about the vertical diameter.

Then, if the stresses are supposed continuous about the point under consideration, we shall have the equations

$$r\frac{\partial P}{\partial r}+2P-Q-R+2Ux-(1-x^2)\frac{\partial U}{\partial x}=g\rho rx,$$

$$\frac{\partial U}{\partial r}-3U+(Q-R)\frac{x}{1-x^2}-\frac{\partial Q}{\partial x}=-g\rho r,$$

$$r\frac{\partial T}{\partial r}+3T-\frac{\partial S}{\partial x}+2S\frac{x}{1-x^2}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

It is clear that S and T do not appear in the first two of these equations, and that we are at liberty to proceed with those two only. To propose that $S=0$, $T=0$, may not be unreasonable; this would require that the tangent to a parallel of latitude at any point in the shell should be a principal axis of the stress quadric at the point, but it is not very material to the procedure at the moment.

For the purpose of engineers, and for practical purposes generally, we have to deal with structural rather than molecular forces. I, therefore, multiply each of these equations by rdr and integrate from $r=a$ at the interior surface of the shell to $r=b$, at the exterior surface, noting that $P=0$, $T=0$, $U=0$ at each such surface.

We thus obtain, writing $\bar{Q}$ for $\int_a^b Qrdr$, $\bar{R}$ for $\int_a^b Rrdr$, $\bar{U}$ for $\int_a^b Urdr$, $\bar{S}$ for $\int_a^b Srdr$, and $\bar{T}$ for $\int Trdr$, and integrating,

$$\bar{Q}=\bar{U}x-\frac{w'}{1+x}+(1-a)w'\frac{1}{1-x^2}$$

$$\bar{R}=\bar{U}x-(1-x^2)\frac{\partial\bar{U}}{\partial x}-wx+\frac{w}{1+x}-(1-a)w'\frac{1}{1-x^2}$$

$$(1-x^2)\bar{T}=\frac{\partial}{\partial x}\{(1-x^2)\bar{S}\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where $w = \frac{g\rho}{3}(a^2 - b^2)$, and the shell is an open shell bounded by the cone for which $x = a$.

The only relations of this character of which I am aware are given in Rankine's *Applied Mechanics*, but I am not sufficiently versed in the subject to be sure that they originate with his treatment.

Rankine's proof offers no difficulty, except in the assessment of his meaning of the phrase "intensity of pressure." If we assume that $\int_a^b Q r dr$ is the measure of the "intensity of the longitudinal pressure," $\int_a^b R dr$ that of "the ring pressure," and also assume that $\bar{U} = 0$, $\bar{S} = 0$, $\bar{T} = 0$, we arrive at Rankine's conclusions, which I gather from various publications by Engineering Institutions in England, Germany, and America, are used in the calculations for the design of domes in concrete as well as for masonry, although Rankine only contemplated dry masonry construction in his book.

A general formal solution of the equations (1) may now be obtained.

Write

$$Q = Ux + \frac{g\rho r(x-a)}{1-x^2} + X.$$

$$R = Ux - (1-x^2)\frac{\partial U}{\partial x} - g\rho r x - \frac{g\rho r(x-a)}{1-x^2} + Y. \quad . \quad (3)$$

Then, by comparison with (2), we must have

$$\int_a^b r X dr = 0,$$

$$\int_a^b r Y dr = 0.$$

Also since Q and U are both to vanish when $x=a$, X must have the form

$$X=\Sigma u\int_a^x v\,dx$$

where u and v are functions of r and x.

On substitution in the general equations of equilibrium we obtain the further equations

$$r\frac{\delta P}{\delta r}+2P=X+Y,$$

$$r\frac{\delta U}{\delta r}+2U=\frac{\delta X}{\delta x}-\frac{x}{1-x^2}(X-Y),$$

whence

$$Pr^2=\int_a^r r(X+Y)\,dr,$$

$$Ur^2=\frac{\delta}{\delta x}\int_a^r rX\,dr-\frac{x}{1-x^2}\int_a^r r(X-Y)\,dr. \quad . \quad . \quad (4)$$

Since U must vanish when $x=a$, we still have to impose a further condition in this last case.

Taking

$$X=\Sigma u\int_a^r v\,dx$$

it appears to be necessary that

$$\int_a^r ruv\,dr+\frac{x}{1-x^2}\int_a^r rY\,dr$$

shall vanish when $x=a$, or that

$$uv+\frac{x}{1-x^2}Y$$

shall have a form similar to that of X.

[We are also enabled to complete the set of equations (2) by finding $\int_a^b Pr\,dr$, and $\int_a^b Ur\,dr$, say $\bar{P}$ and $\bar{U}$.

Thus

$$\bar{P}=-\int_a^b r\log r(X+Y)\,dr$$

and

$$\bar{U}=-\int_a^b r\log r\frac{\delta X}{\delta x}\,dr+\frac{x}{1-x^2}\left\{\int_a^b r\log r(X-Y)\,dr\right\}.\Big]$$

The equations of condition, of which the importance is wished to be pointed out, may be written

$$q\frac{\partial u}{\partial r} = P - \sigma(Q+R),$$

$$q\left(u + x\frac{\partial v}{\partial x}\right) = r\{R - \sigma(P+Q)\},$$

$$q(1-x^2)\frac{\partial^2 v}{\partial x^2} = -(1+\sigma)r(Q-R),$$

$$q\frac{\partial}{\partial x}\left(r\frac{\partial v}{\partial r} - v - u\right) = 2(1+\sigma)U. \quad . \quad . \quad . \quad . \quad (5)$$

where it is more convenient to use the elastic constants q and σ than m and n, and where

$$\sigma = \frac{m-n}{2m}, \quad q = 2(1+\sigma)n.$$

From these equations we might eliminate u and v, but the results are very complex.

We have already found the mathematical form which u and v possess if the equations are consistent, and these values would exclude the gravitational portion of the stress which is given by

$$Q = g\rho r\frac{x-a}{1-x^2},$$

$$R = -g\rho r\left(x + \frac{x-a}{1-x^2}\right).$$

Stress in a long, heavy circular cylindrical tube, supported horizontally.

Take the interior and exterior radii as a and b, and assume the tube to be so long that the effects of the ends may be neglected, the statical equations to be satisfied are

$$r\frac{\partial P}{\partial r} + P + \frac{\partial U}{\partial \theta} - Q = g\rho r\cos\theta.$$

$$r\frac{\partial U}{\partial r} + 2U + \frac{\partial Q}{\partial \theta} = -g\rho r\sin\theta. \quad . \quad . \quad . \quad . \quad (6)$$

The stresses across the sections of the tube inclined θ to the vertical must support the weight of the included portion of the tube.

The formal solution will therefore be found from

$$P = p\theta \sin\theta + p_0 \cos\theta + \Sigma p_s \cos s\theta ,$$

$$Q = \left(r\frac{\delta p}{\delta r} + 2p\right)\theta \sin\theta + \left(r\frac{\delta p}{\delta r} + 2p + g\rho r\right)\cos\theta + \Sigma q_s \cos s\theta ,$$

$$U = -p\theta \cos\theta + \Sigma u_s \sin s\theta ,$$

where $p = 0$, when $r = a$, and when $r = b$, and

$$\int_a^b p\,dr = -\tfrac{1}{2} g\rho(b^2 - a^2) ,$$

$$rp_0 = rp + 2\int_a^r p\,dr + g\rho(r^2 - a^2) ,$$

and, in the series of complementary terms,

$$s\,q_s = r\frac{\delta u_s}{\delta r} + 2u_s ,$$

$$s\left(r\frac{\delta p_s}{\delta r} + p_s\right) = r\frac{\delta u_s}{\delta r} - (s^2 - 2)\,u_s .$$

The equations of condition in terms of displacements are now,

$$q\frac{\delta u}{\delta r} = P - \sigma Q ,$$

$$q\left(u + \frac{\delta v}{\delta \theta}\right) = r(Q - \sigma P) ,$$

$$q\left(r\frac{\delta v}{\delta r} - v + \frac{\delta u}{\delta \theta}\right) = 2r(1 + \sigma)U . \quad . \quad . \quad . \quad . \quad (7)$$

From these, again, the displacements might be eliminated. But, if instead, we examine the solution of the displacement equations, namely,

$$(m + n)\,r\frac{\delta \Delta}{\delta r} - 2n\frac{\delta \omega}{\delta \theta} = g\rho r \cos\theta ,$$

$$(m + n)\frac{\delta \Delta}{\delta \theta} + 2nr\frac{\delta \omega}{\delta r} = -g\rho r \sin\theta ,$$

where

$$\Delta = \frac{\delta u}{\delta r} + \frac{u}{r} + \frac{1}{r}\frac{\delta v}{\delta \theta},$$

and

$$2\omega = \frac{\delta v}{\delta r} + \frac{v}{r} - \frac{1}{r}\frac{\delta u}{\delta \theta},$$

and if we consider separately the terms

$u_0\theta \sin\theta$ and $v_0\theta\cos\theta$ in u and v respectively,

then

$$r\frac{\delta u_0}{\delta r} + u_0 - v_0 = (1-2\sigma)(Ar^2 + B),$$

$$r\frac{\delta v_0}{\delta r} + v_0 - u_0 = -2(1-\sigma)(Ar^2 - B),$$

from the displacement equations; and the conditions are that

$$q\left(r\frac{\delta v_0}{\delta r} - v_0 + u_0\right) = -2(1+\sigma)rp,$$

$$q\left(r\frac{\delta u_0}{\delta r} + \sigma(u_0 - v_0)\right) = (1-\sigma^2)rp,$$

$$q\left\{\sigma r\frac{\delta u_0}{\delta r} + (u_0 - v_0)\right\} = (1-\sigma^2)\frac{\delta}{\delta r}(pr^2) \quad . \quad . \quad . \quad (8)$$

The different conditions implied by these equations cannot be satisfied, it being remembered also that p vanishes both when $r=a$ and $r=b$.

The general conclusion at which I arrive is that the stresses in a heavy body cannot reasonably be assumed to be elastic stresses, that the conditions required in order that the stresses should be elastic are very stringent, and in the cases I have examined the stresses do not satisfy these conditions.

In the case of the Earth, which offers great interest, the magnitudes of the stresses make it especially urgent that the assumption that the stresses are elastic should not be made without a very thorough investigation.

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XXI. Dioptriemeters.

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and

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In 1872 Monoyer* proposed the term *dioptrie* as the unit of focal power. It represents the focal power of a lens one metre in focal length, and since the focal powers are the reciprocals of the focal lengths, a lens of two metres focal length will be half of a dioptrie, a lens of half a metre focal length will be two dioptries, and so on. This unit proved of so great a convenience in practical optics that it was finally adopted in 1875 by the International Medical Congress at Brussels, and by the International Congress of Ophthalmology at Heidelberg. Unfortunately, a uniformity of spelling has not been followed, so that we find the unit is also given the names of *dioptry*, *dioptre*, and *diopter*. The term *dioptric* for the unit is also used by A. Bruce.† Since some of these varieties have other meanings, it would be desirable if the original spelling were followed, so as to conform to French and German usage. S. P. Thompson has proposed to extend the use of the *dioptrie* for the measurement of curved surfaces in general.

* "Annales d'Oculistique," vol. 68, p. 111.

† "Encyl. Brit.," vol. 22, p. 375, 1887.

June 1st, 1911.

In the case of lenses where great accuracy is not required, in converting from inches into dioptries a metre is taken as 40 inches, thus we have :—

Focal Length. Inches.	Focal Length. Metres.	Dioptries. Actual.	Dioptries. Approximate.
1	0·0254	39·37	40
5	0·1270	7·87	8

Generally, if the focal length be f metres and its power D in dioptries, then numerically $D = 1/f$. The sign of D is onsidered as positive for converging, and negative for

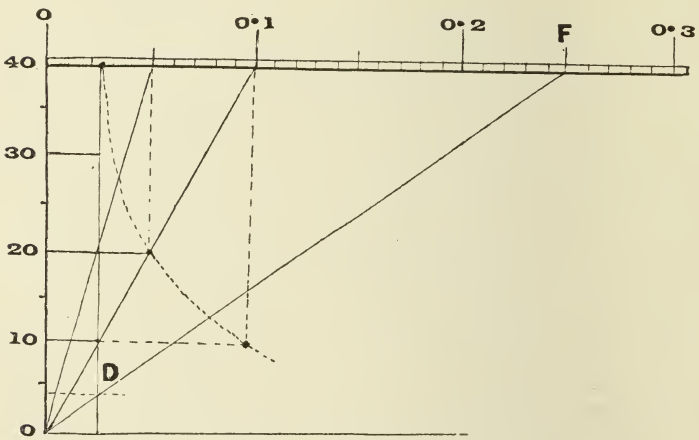

Fig. 1.—First Graphical Method for finding D.

diverging lenses; whilst in the convention of signs used by most writers on geometrical optics the signs of the focal lengths of the corresponding lenses are used in the reverse way. If both conventions are adopted, the algebraical relations between D and f is $D = -1/f$. When f is known, the value of the reciprocal D can be found by calculation, by using a table of reciprocals, or by a graphical construction—such as shown in *Figs.* 1 and 2—where the value of D may be read directly on any scale of equal parts.

The horizontal scale in *Fig.* 1 represents a metre scale divided into tenths, whilst the vertical scale is one of any equal parts, say 40, so that the line passing through D (this may be called the *dioptrie line*) passes through 1/40 on the horizontal scale. From the zero of the vertical scale a line is drawn to the principal focus F of a lens supposed to be at the zero of the horizontal scale. The length of the intercept at D—in the diagram =4—gives the focal power. It will be noted that the focal lengths and the corresponding dioptries are co-ordinates of points on the hyperbola $xy = 1$.

A second graphical method is shown in *Fig.* 2. The horizontal scale is divided into ten equal parts. They represent, but need not be equal to tenths of a metre. On this scale AF represents the focal length. A line is drawn through the centre of the lens and at right angles to the axis. From this is cut off any arbitrary length AB to serve as a unit. At the other end of the horizontal scale a scale of dioptries is set up with AB as a unit. This scale is numbered as shown. On drawing a line from B through F the value of the intercept at D on the vertical scale will give the dioptries. In the diagram this value is 2·5, which is equal 1/0·4. The method is easily proved for

$$AB/AF = \tan AFB = \tan DBO = OD/OB = D \cdot AB/\text{unity}$$

or

$$1/AF = D.$$

The actual determination of f for a thin converging lens presents no difficulty, for several optical-bank methods give the value directly. The use of real conjugate foci is involved in other methods. If the distances of object and real image from the lens be u and v metres respectively, then numerically :—

$$1/u + 1/v = 1/f = D.$$

A graphical construction for finding D from u and v is shown in *Fig.* 3.

In this construction the lengths u and v are marked off on each side of the lens A on a scale representing tenths of a metre, and as before an arbitrary unit AB is

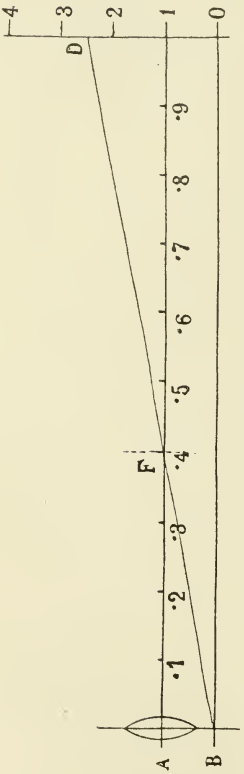

Fig. 2.—Second Graphical Method for finding D.

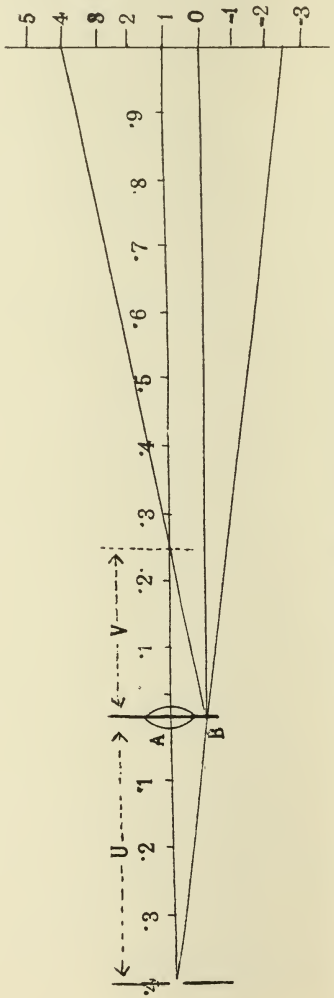

Fig. 3.—Third Graphical Method or finding D.

chosen for the scale of dioptries. The scale reading below 0 will represent the value $1/u$ and the upper scale

reading will give $1/v$, hence their numerical sum will be $1/f$. In the example shown $u = 0{\cdot}4$, and $1/u = 2{\cdot}5$; $v = 0{\cdot}25$ and $1/v = 4$. Hence $D = 6{\cdot}5$.

For concave lenses there is more difficulty in finding focal length, for there are no good direct methods available. The best way is to combine the concave lens with a more powerful convex lens. In optological practice this is accomplished by the help of a box of lenses—called a "test case"—from which a convex lens can be selected so as to neutralise the negative lens.

A very useful and interesting method of investigating the power of a lens is to observe a uniform scale through a small pin-hole, and to notice the displacement of the scale divisions when a lens with a stop of definite aperture is interposed. The use of the pin-hole prevents errors due to parallax and no focussing is required, but the scale must be well illuminated. Since the images formed by the lens are not observed, only the deviation of the rays being measured, the method is applicable to both converging and diverging lenses. The power in dioptries may be read off directly from any uniform scale in the following way: Observe the scale A (*Fig.* 4) through a pin-hole H and a circular aperture EG in a disc DD. Let the radius of the aperture be a scale divisions, the distance from the pin-hole H to the centre C of the aperture be y metres, and the distance from C to the centre N of the scale x metres. The point M of the scale will be on the boundary of the field of view, and the number of scale divisions visible on each side of N will be :—

$$a\mathrm{HN}/\mathrm{HC} = a(x+y)/y.$$

Let the scale division at M be marked zero. Place the lens to be tested in contact with the disc DD, with its axis in the line HCN, then the rays of light entering H

from the margin at E of the aperture will have come from the point P of the scale. In the diagram the dotted lines correspond to a concave lens. The deviation of the ray produced by the lens will be the angle PEM. If the scale reading corresponding to the point P be S scale divisions

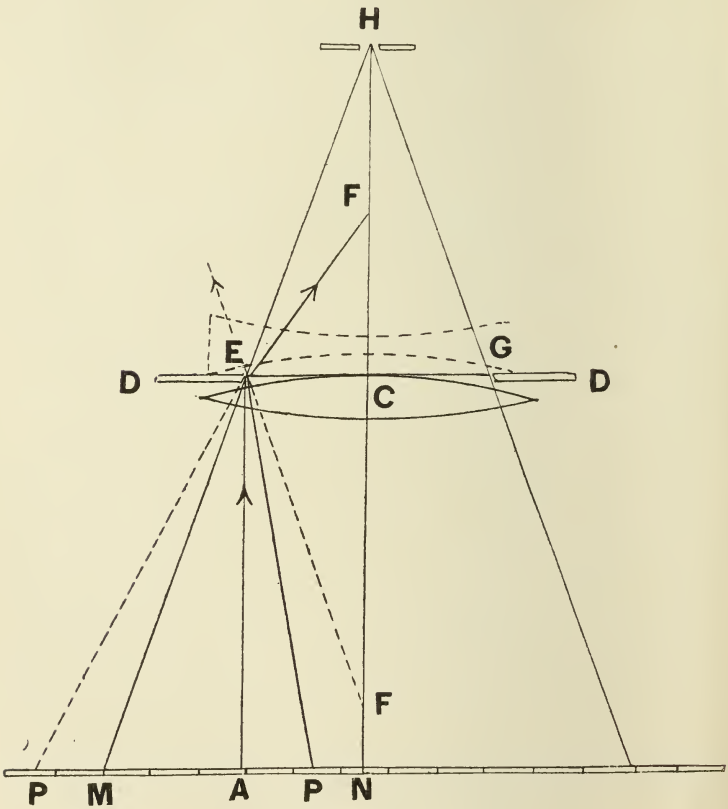

Fig. 4.—Theory of Direct Method.

(measured from the zero point M) the angular deviation is given approximately by the ratio of the lengths :—

$$MP/EP = MP/CN = S \text{ scale divisions} / x \text{ metres.}$$

This angular deviation is nearly the same as the deviation of a ray of light originally parallel to the axis of

the lens, when passing through it at a distance a scale divisions from the centre. It is, therefore, approximately measured by the ratio EC/FC or by a scale divisions divided by f metres, where f metres is the focal length of the lens.

Thus we have :—

$$\frac{\text{S scale divisions}}{x \text{ metres}} = \frac{a \text{ scale divisions}}{f \text{ metres}}$$

or

$$S/a = x/f.$$

If we now agree to make the radius a of the aperture equal to $1/x$ scale divisions, then :

$$S = 1/f = D$$

where D is the power of the lens in dioptries.

The number of scale divisions from the zero division M to the centre N now becomes $1/x + 1/y$.

The choice of a scale to be used in this method will depend upon the range of focal powers to be tested, and the radius of the aperture must not be too great for the size of lens to be used. An ordinary millimetre scale can be used if the distance $x+y$ is not too great.

It is an advantage to use a scale made in the form of concentric circles with N as centre, so that the power of the lens in different planes can be read off without rotating the scale.

This was the method adopted by Dr. Guilloz, Professor in the Faculty of Medicine at Nancy, and was described in 1895 to the Congrès pour l'avancement des Sciences at Bordeaux. The type of focometer required for the purpose was made by Pellin (the successor of Duboscq). We have simplified the instrument and modified it so that it can be cheaply constructed. We think that in the new form it will be of considerable service in optical measurements, and will help in making students familiar with the use of the dioptrie.

The instrument—which may be called a Dioptriemeter—is shown in *Fig.* 5. It consists of a wooden upright supported on a wooden base. On the latter is fixed a horizontal card, marked with a series of concentric circles 1 mm. apart, the radius of the innermost circle being 5 mm., and that of the outermost 30 mm. A thin disc of metal, with a circular aperture of 10 mm. radius, is supported exactly 100 mm. above the card. Another thin disc of metal, having a central pin-hole, is fixed exactly 200 mm. above the card. The centre of the circular opening is

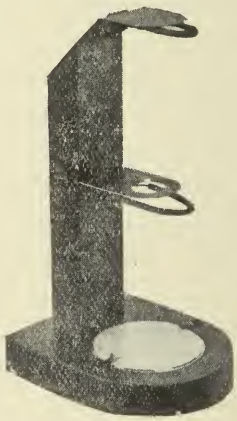

Fig. 5.—The Dioptriemeter.

indicated by cross wires, and the centre of the concentric circles is marked by a cross. These two centres and the pin-hole lie in the same vertical line, which is the axis of the instrument.

When the eye is placed close to the pin-hole the field of view limited by the circular aperture will obviously include all the circles up to that of 20 mm. radius. This circle is called the zero circle, and is marked 0. The other circles are numbered consecutively, those inwards being marked +, those outwards −.

The lens whose power is to be determined is placed as near as possible in contact with the disc. If the lens is convex it should be placed in the holder, which will press it against the disc. If the lens be concave, it is simply laid on the disc. The lens must be adjusted so that on looking through the pin-hole the centres of the aperture and the circles appear to coincide. The number of circles now in the field of view (*Fig.* 6) depends upon

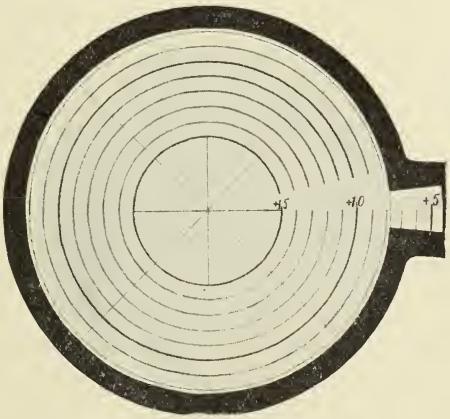

Fig. 6.—Scale of Dioptriemeter seen through a convex spherical lens.

the power of the lens, and the scale reading of the outer circle seen gives the value of the power of the lens at once in dioptries. To facilitate the estimation of the fractions of a dioptrie, a portion of the disc at one side of the aperture is cut away, so as to expose a portion of the circles which would otherwise be hidden, and a fine wire is stretched across the gap at the place where the readings must be taken ; in *Fig.* 6 the value is seen to be +7·8 D.

The theory of the method follows from the formulæ already given. Since the radius of the aperture is 10 mm. or 10 scale divisions, and the distance from the aperture

to the scale is 1/10 metre, we have, following the notation already used :—

$$x = 1/10$$

and $a = 10$ scale divisions

$$= 1/x.$$

Thus the required condition is fulfilled, so that S = D. Also, the radius of the zero circle being 20 mm., is equal to $1/x + 1/y$.

Hence, the power of the lens can be read off directly, as already explained, and it will be evident that the scale divisions marked + will correspond to convex lenses and the − divisions to concave lenses.

In proving that the readings on the scale of the instrument are the dioptric powers of the lenses tested, the following assumptions and approximations are made:

(1) It is assumed that rays passing through a lens at a fixed distance from the centre are equally deviated.

(2) It is assumed that the tangent of the angle of deviation is equal to the sum (or difference) of the tangents of two angles whose sum (or difference) is the angle of deviation.

(3) It is assumed that the point of intersection of the incident and emerging rays of light is in the plane of the aperture.

These assumptions will only be strictly accurate if the lens is indefinitely thin, and if the rays of light pass through the lens indefinitely near its centre. The instrument is therefore only approximately correct in principle, the errors being greater when the lens is thicker and more powerful.

The ordinary methods of determining the focal lengths of thin lenses by means of the optical bank are also inaccurate in principle, if the aperture and thickness of the lens are not indefinitely small.

We have compared the results for the power and focal length of a double convex lens of symmetrical form of about 15 D. by calculation from the usual optical bank formulæ with the dioptriemeter value. In each case the rays of light are considered to pass through the lens at a distance of one cm. from its centre.

Let f_1 be the distance from the centre of the lens to the principal focus, that is, to the point of convergence of rays originally parallel to the axis of the lens, and one cm. from the axis.

Let f_2 be $\frac{1}{4}$ of the distance between a small object and its real image, when these are equidistant from the centre of the lens and of equal size.

Let f_3 be the reciprocal of the power as given by an accurate reading on the dioptriemeter.

The thickness of the lens was 5 mm. at one cm. from its centre, and it was assumed that $(\mu-1)(2/R)=15$, where μ = refractive index and R = radius of curvature of each face in metres. The result of the calculations, the errors referred to being corrected to a first degree of approximation, give :—

$f_1 = 0{\cdot}0685$ m.	$D_1 = 14{\cdot}60$.
$f_2 = 0{\cdot}0681$ m.	$D_2 = 14{\cdot}67$.
$f_3 = 0{\cdot}0681$ m.	$D_3 = 14{\cdot}68$.

A similar comparison between f_1 and f_3 was made in the case of a double concave lens of about -10 D., the thickness at 1 cm. from the centre being 2 mm. Here, assuming $(\mu-1)(2/R)=10$, it was calculated that $1/f_1 = 10{\cdot}01$, and $1/f_3 = 10{\cdot}07$.

There is usually no difficulty for a normal eye to see the scale distinctly with any power of lens that can be used, but if this should not be the case, a suitable focussing lens can be placed in the lens holder just under the

pin-hole disc. Such a lens will make no appreciable difference in the scale reading.

An important use of the instrument is for the testing of cylindrical and sphero-cylindrical lenses. When such lenses are used the circles appear distorted into curves somewhat resembling ellipses. With a cylindrical lens, the polar equation is of the form :

$$\frac{1}{r} = \frac{\cos^2\theta}{a} + \frac{\sin^2\theta}{b}$$

where a and b are the semi-axes.

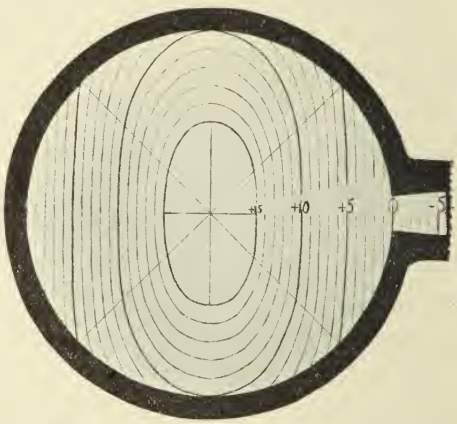

Fig. 7.—Scale of Dioptriemeter seen through a convex cylindrical lens.

The equation to the ellipse with the same semi-axes is :

$$\frac{1}{r^2} = \frac{\cos^2\theta}{a^2} + \frac{\sin^2\theta}{b^2}.$$

The power of the lens in any plane, or meridian, can be determined by observing the scale number of the elliptical type of figure which intersects the boundary of the field of view in that plane, or, better, by rotating the lens until the selected plane intersects the wire that is used for taking accurate readings.

With a cylindrical lens, either the major or minor axis of the figure marked zero will just extend across the field of view, and the direction of this axis will coincide with the direction of the axis of the cylinder, which can thus be found.

The effect of combining two cylindrical lenses with the axes of the cylinders at any angle, can readily be shown and the power of the combination in different planes determined. For example, if a pair of cylindrical lenses of the same power and sign be placed, one above the aperture and the other below, with the axes of the cylinders crossed at right angles, the appearance shown will be that of concentric circles, proving that a combination of this kind is equivalent to a spherical lens of the same power.*

The dioptriemeter is also useful for showing that the power of a combination of thin lenses in close contact is the algebraical sum of the powers of the separate lenses. In determining the power of a concave lens which is beyond -10 D, or a convex lens beyond $+15$ D, the method of combination may be applied. This is effected by placing the lens in its proper position according to sign and combining it with a suitable lens. The power of the combination having been read off, the power of the lens under test is found by subtraction after finding the power of the auxiliary lens. The value so obtained is but approximate, for the thickness of the lens and its distance from the aperture prevent accuracy.

When a convex lens of 14 to 15 D is in position, the accuracy with which the scale can be read is much greater owing to the magnification. Readings can then easily be taken to 1/5 D or about 1·4%. The focal length

* For other examples see S. P. Thompson "On Obliquely crossed Cylindrical Lenses," *Phil. Mag.*, vol. 49, p. 316, 1900.

of such a lens is about 70 mm., and this the instrument will give correctly to the nearest millimetre. With weak lenses, either concave or convex, of about 1 D, the readings cannot be taken with certainty to less than 1/4 D. With concave lenses of 10 D, the circles are so diminished that the readings can only be estimated to ½ D, or 5%. It is better for concave lenses of this kind, or weak convex lenses, to combine with +14 or +15. The auxiliary lens should be placed in the holder below the aperture so as to bring the readings to a better part of the scale.

The angle of minimum deviation caused by a thin prism or wedge can readily be found by the dioptriemeter. When the prism is placed above or below the aperture, the system of circles is seen displaced to one side. If the prism be rotated about a vertical axis, the circles appear to move round the intersection of the cross wires as a centre. Let the reading on the scale corresponding to this fixed point be P, then the tangent of the angle of deviation will be (20-P)/100.

A prism which causes a minimum deviation of one centimetre when measured at a distance of one metre is said to have a power of one *prism dioptrie.* By the use of the dioptriemeter the deviation of 20-P mm. measured at a distance of one-tenth of a metre from the prism is clearly the same as a deviation of 20-P cm. measured at one metre, and thus corresponds to a power of 20-P prism dioptries.

The instrument may also be used to localise any irregularity in the curvature of a lens at any point. This would be shown, if sufficiently great, by a local distortion of the circles.

When a slab of glass of thickness t is placed at an angle to the axis of the instrument, then the centre of

the rings will be laterally displaced a distance d. If r be the angle of refraction we have :

$$\tan r = \tan i - d/t \cos i.$$
$$\text{and } \mu = \sin i/\sin r$$

from which μ the index of refraction can be calculated. Thus if $i = 45°$, $t = 47{\cdot}6$, and $d = 15{\cdot}5$ then $\mu = 1{\cdot}5$ nearly.

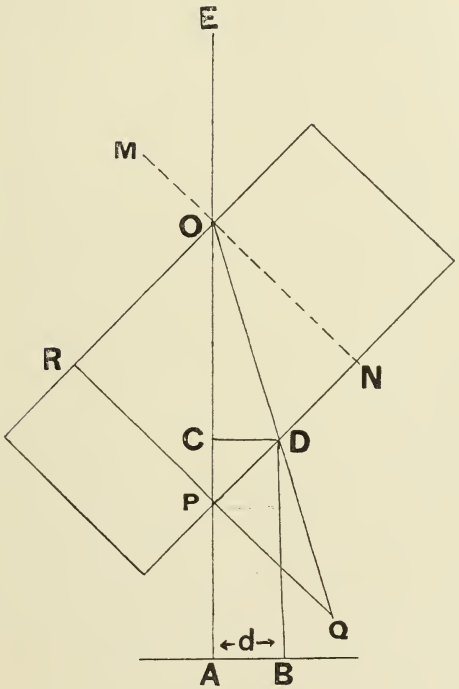

Fig. 8.—Displacement produced by glass slab.

The calculation being rather involved, a graphical method may be applied as shown in *Fig.* 8, the ratio OQ/OP giving the value of μ.

If the slab of glass be replaced by a lens, the apparent distortion of the circles will vary with the angle i. A

study of the figures produced is interesting in connection with the geometrical optics of oblique pencils.

In conclusion, we have to express our thanks to G. Cussons Ltd., The Technical Works, Broughton, Manchester, for the care taken in constructing the dioptriemeter, and to the Principal and Committee of the Manchester School of Technology, for the facilities placed at our disposal.

Manchester Memoirs, Vol. lv. (1911), *No.* 22.

XXII. The Development of the Atomic Theory: (7) The rival claims of William Higgins and John Dalton.

By Andrew Norman Meldrum, D.Sc.

(Communicated by Mr. R. L. Taylor, F.C.S., F.I.C.)

Received March 22nd, 1911. Read April 25th, 1911.

The rival claims of William Higgins and John Dalton to the atomic theory were much discussed early in the nineteenth century. The result of the discussion was, on the whole, favourable to Higgins. But from a variety of reasons, this result has been forgotten, and Dalton's claims are supposed at the present time to be beyond dispute. The author, in reviving the subject, hopes to present the facts, and to offer considerations, so as to enable anyone interested to come to a simple and fair conclusion upon it.

With the above purpose in view, it is necessary to consider the question, What is the essence of Dalton's atomic theory? This question, one of much interest, has proved in the experience of writers on the subject, one also of much difficulty. An answer to it, which cannot be set aside, has recently been given by Larmor. In his Wilde Lecture—"On the Physical Aspects of the Atomic Theory"—he has expressed the Daltonian principle in the words "a definite molecule for each substance." He explains this more fully as follows:—"Perhaps the new feature developed by Dalton is at bottom describable as the principle of

the essential homogeneity of each pure substance, that it is composed of molecules of only one type, absolutely alike. Once it is postulated that only one kind of aggregation into molecules occurs, *e.g.*, that in water there is only one way in which the hydrogen attaches itself to the oxygen, the laws of definite and multiple proportions are self-evident."[1]

Undoubtedly this principle, "a definite molecule for each substance," is common to the various systems of chemistry of the nineteenth century. Yet the principle was not necessarily advanced first by Dalton. I have already shown (in the third paper of this series) that William Higgins expounded a definite chemical atomic theory in a book which he published in the year 1789. Further, the words, a "definite molecule for each substance," give, as will presently appear, an unexceptionable statement of the theory contained in Higgins' book.

The two theories, Higgins' and Dalton's, led their authors, in a remarkable degree, to the same results. This is proved by the following table, the formulæ in which reveal at once the ideas which Higgins and Dalton had regarding the molecules of the substances in question:

	Higgins (1789).	Dalton (1803).
Water	HO	HO
Ammonia	—	HN
Oxides of sulphur ...	SO and SO_2	SO and SO_2
,, carbon ...	—	CO and CO_2
,, nitrogen...	NO, NO_2, NO_3, NO_4 and NO_5	N_2O, NO and NO_2

The great similarity between these results is to be explained in only one way. The two theories have in common, as a guiding principle, the rule that atoms of

[1] *Manchester Memoirs*, 1908, **52**, No. 10, p. 9.

different kinds combine in the proportion 1 : 1 rather than in any other. It was this rule, and no other, which led each chemist to precisely the same conclusions regarding water, and the oxides of sulphur, respectively.

How the rule was arrived at is a matter of the historical origin of the theories. As I have already shown, they arose from the same central capital idea: Newton's postulate of "particles mutually repulsive" was the starting point in each case. The thoughts of each chemist ran in the same groove. Similar particles repel one another, consequently particles of different kinds tend to unite in pairs.

Bryan Higgins was the first to reach this stage of thought, and he would not depart from it in any way. He supposed that the combination of one atom of alkali and two atoms of acid (or two of alkali and one of acid) must be prevented by the mutual repulsion of the two similar atoms, so that combination could not proceed further than 1 : 1.

Better acquainted than he with the facts of chemical combination, William Higgins imagined the combination of atoms in multiple proportion. But he laid it down that the combination in the proportion 1 : 1 was the most stable, thus adhering to the original idea of mutually repulsive particles.

The train of thought which Dalton followed had features of its own. His physical atomic theory was plainly an extension of Newton's, and was called for by the discovery of the existence of different gases, of their property of diffusing into one another, and of the properties of the resulting mixture. As I have shown in the fifth paper of this series, he held the physical theory for two years before he formed the chemical one. He was able

to devise the latter in the space of a month, simply because he had the former to work upon.

The close resemblance between the two theories, both in principle and results, puts it beyond doubt that Dalton was forestalled by William Higgins. Humphry Davy, in his Bakerian Lecture of the year 1810, was the first to draw attention to Higgins' claims. The terms in which he did so are remarkably decided, and such as to throw him into almost too pronounced antagonism to Dalton. "In my last communication to the [Royal] Society, I have quoted Mr. Dalton as the original author of the hypothesis that water consists of 1 particle of oxygen and 1 of hydrogen, but I have since found that this opinion is advanced in a work published in 1789—'A Comparative View of the Phlogistic and Antiphlogistic Theories,' by William Higgins. In this elaborate and ingenious performance, Mr. Higgins has developed many happy sketches of the manner in which (on the corpuscular hypothesis) the particles or molecules of bodies may be conceived to combine; and some of his views, though formed at this early period of investigation, appear to me to be more defensible, assuming his data, than any which have been since advanced."[2]

The only public notice which Dalton himself took of Davy's words was to publish a paper in which he was careful not to name Higgins. The date of Davy's lecture was 15th November, and of Dalton's paper 19th December. He contended that the use of the word *particle*, as opposed to *atom*, was a matter of great consequence—a contention which was quite unworthy of him.[3]

[2] Bakerian Lecture, 15th Nov., 1810, *Phil. Trans.*, 1811, p. 15; Davy's Works, vol. 5, p. 326.

[3] *Nicholson's Journ.*, vol. 28, p. 81, 1811.

Though Davy saw fit afterwards to qualify this declaration, he could never undo the effect it produced. No one was better able than he to make Higgins known at once, for he was famous throughout Europe. His papers were read by all scientific men: thus Berzelius and Arago each mention that their attention was drawn to Higgins by Davy. The consequence in this country was a long and desultory controversy regarding the respective claims to the atomic theory of William Higgins and John Dalton.

In the course of the controversy the suggestion was made that Dalton had been guilty of plagiarism at the expense of Higgins. The charge was made far too lightly.[4] Dalton was not a great reader, and it was very unlikely he would look twice at a book which dealt, on the face of it, expressly with the phlogiston controversy. But it was necessary that a statement on the subject should be made, and the statement was forthcoming. Thomas Thomson declared that Dalton had no knowledge of Higgins' book previous to the year 1810, and this declaration was made repeatedly afterwards by Dalton's personal friends.[5]

It is easy to account for the resemblance between the two theories. They had a common origin in Newton's ideas, and there is no need for any other explanation.

[4] Du Bois Reymond and Helmholtz each hit upon the same illustration of the time taken by a nervous impulse. "A whale probably feels a wound near its tail in about a second, and requires another second to send back orders to the tail to defend itself." "Hermann von Helmholtz," by von Kœnigsberger, Eng. trans., 1906, p. 72.

[5] Thomson, *Annals of Phil.*, 4, 54, 1814; William Henry, "Elements of Experimental Chemistry," 11th ed., 1829, 1, 45; W. C. Henry, "Memoirs of Dalton," pp. 78, 175, 217.

Thomas Thomson was Dalton's champion during the controversy, and he stoutly resisted Higgins' claims. The position he adopted is a specially interesting one. He declared that although Higgins' book had been widely read, no one had perceived the atomic theory in it. He therefore denied that the theory was there. This is not an answer, however, but an argument, and one that Thomson could hardly have used if he had kept in mind the reception Dalton's theory met with when it was launched upon the world. (See the sixth paper of this series.)

Humphry Davy, for instance, ignored it for long, and disparaged it when it was forced upon his notice. One can hardly wonder, then, that Higgins' speculations should have been disregarded, for they appeared many years before, under cover of a contribution to the phlogiston controversy.

Thomson, however, offered the testimony that he himself had failed to perceive the theory in Higgins' book. "I have certainly affirmed that what I consider as the atomic theory was not established in Mr. Higgins' book... I have had that book in my possession since the year 1798, and had perused it carefully; yet I did not find anything in it which suggested to me the atomic theory. That a small hint would have been sufficient I think pretty clear from this, that I was forcibly struck with Mr. Dalton's statement in 1804, though it did not fill half an octavo page."[6]

This is hardly enough to establish Thomson's case. It amounts to the plea that he was not making a mistake in the year 1814, simply because he could not have made it in the year 1798. Charles Darwin was a humbler

[6] *Annals of Phil.*, 3, 331, 1814.

man. As mentioned in the fifth paper of this series, he confessed that he and Sidgwick once passed along a valley without observing signs of glacial action, which were, none the less, present everywhere. They failed to perceive these signs because they were directing their attention to something else. In the same way the atomic theory may be in Higgins' book even though Thomson failed to perceive it.

As a result of the controversy, it appeared that most chemists were unable to deny Higgins' claims. William Hyde Wollaston observed that Mr. Higgins "in his conception of union by ultimate particles clearly preceded Mr. Dalton in his atomic views of chemical combination."[7] Thomas Graham, again, in his "Chemical Catechism," puts the question, "Who first made use of the atomic hypothesis in chemical reasonings?" The answer is:— "A Mr. Higgins, of Dublin—in a book of his published in the year 1789."[8]

Again, it is true that William Higgins has been almost forgotten. After his death, in 1825, he gradually passed out of notice and recollection. The claims of Dalton, on the other hand, have been advocated by a succession of Manchester chemists, including W. C. Henry, R. Angus Smith, and Roscoe and Schorlemmer. These writers have thought to advance their cause by disparaging Higgins, but, as I have shown in the third paper of this series, their criticisms are unfair, and must be set aside.

As I have already said, inasmuch as the "Daltonian principle, a definite molecule for each substance," is the principle also of Higgins' theory of the year 1789, there is no avoiding the conclusion that Higgins forestalled

[7] *Phil. Trans.*, 1814, p. 5.

[8] "Chemical Catechism," 1829, p. 35.

Dalton. This is no small merit, for the said principle is the central idea of all the atomic weight systems of the nineteenth century.

It is, however, a great mistake to suppose that this conclusion exhausts the subject of the merits of the two chemists. Humphry Davy, years after his declaration on behalf of William Higgins, saw that there was something more to be said. He recognised the claims of Bryan Higgins :—" It is difficult not to allow the merits of prior conception, as well as of very ingenious illustration, to the elder writer." [9] He said, further, " Let the merit of discovery be bestowed where it is due, and Mr. Dalton will be still pre-eminent in the history of the theory of definite proportions." [10]

It is true on the one hand, that William Higgins was much indebted to Bryan Higgins, and on the other hand, that he left the atomic theory capable of infinite development by other chemists. It can be urged against him that he did not work out the practical consequences of his ideas. Why did he not make use of his ideas as a guide in experimental work? It might be supposed that he did not attach much importance to them, or he would surely have made strenuous exertions to establish them experimentally, and to make them known. But, as a matter of fact, there is nothing in his writings to show that he had anything but a high opinion of his theory. In the year 1799, seizing the opportunity of the publication of a book of his on bleaching, to draw attention to his system of chemistry, he declared that he had "connected the whole, and reduced it to a system, and made use of demonstrations which, in his opinion, are not to be invalidated or contra-

[9] Davy's Works, 7, 93.

[10] *Op. cit.*, p. 96.

dicted, until the order of natural things assume a different aspect."[11]

The strain of thought here is exalted enough to raise a smile, and, moreover, to prove the high value that Higgins set on his speculations. Elsewhere he stated that he taught the atomic theory in his lectures at the Royal Dublin Society. "What is called the atomic theory formed a part of my annual course of lectures."[12]

Higgins thus exemplifies "faith without works." He had splendid ideas which he did not work out. More than one writer commented on this. Wollaston remarked that "he appears not to have taken much pains to ascertain the actual prevalence of that law of multiple proportions by which the atomic theory is best supported."[13] Davy, in his obituary notice of Higgins, passed a very severe judgment upon him: ". . . . it is impossible not to regret that he did not establish principles which belong to the highest department of chemistry, and that he suffered so fertile and promising a field of science to be entirely cultivated by others; for though possessed of great means of improving chemistry, he did little or nothing during the last thirty years of his life."[14]

William Higgins (saving his indebtedness to Bryan Higgins) stands in much the same relation to the chemical atomic theory as J. A. R. Newlands to the periodic system of the elements. Newlands foreshadowed the periodic system in its most important features. Although his ideas were scouted by the officials of the Chemical Society of London, he adhered to them, and was enabled to publish

[11] "An Essay on the Theory of Bleaching," 1799, p. xx.

[12] *Phil. Mag.*, 1819, 53, 405.

[13] *Phil. Trans.*, 1814, p. 5.

[14] Davy's Works, 7, 75.

them by the open-mindedness of the Editor of the *Chemical News.* In Higgins' case, as in Newland's, we find an idea of extraordinary potency advanced by a man, who for some reason or other, leaves the idea almost in the germ and capable of infinite development by the efforts of others. Moreover, it was not through Higgins and Newlands that these ideas came to have an influence on the progress of science.

Granting the utmost that can be said on behalf of Higgins, one must admit that Dalton made a great contribution to the development of the atomic theory. Much the superior of Higgins in energy of character and mind, he made himself a prime factor in the development of the theory by the persistency of his efforts to extend and apply it in all directions, and to bring it into currency amongst men of science. He applied it first to physical and then to chemical phenomena. In opposition to Berthollet's erroneous teaching regarding mixed gases and the composition of chemical substances, he offered sound ideas based on the theory. Again, he perceived, far more clearly than Higgins, the practical consequences of the combination of atoms. He never delayed putting his ideas to the test of experiment. The test was often hastily and crudely made, so urgently did he feel the necessity of making it. No one can say that the chemical atomic theory was accurately verified by Dalton, and no one can deny that his table of atomic weights brought the theory into touch with facts, and showed to all with eyes to see, exactly what the theory meant. It has already been shown, in the sixth paper of the series, that Dalton converted Thomas Thomson to the theory, that Thomson influenced William Hyde Wollaston, and Amadeo Avogadro, and that Wollaston influenced J. J. Berzelius.

In preparing this series of papers, I made constant use of the "Royal Society Catalogue of Scientific Papers," and the "Select Bibliography of Chemistry," published by the Smithsonian Institution.[15] On bringing the series to an end, I desire to acknowledge my indebtedness to the libraries of the following institutions: The Victoria University of Manchester, Aberdeen University, The Manchester Literary and Philosophical Society, and The Glasgow and West of Scotland Technical College. At the same time I would tender my most grateful thanks to the respective librarians, Mr. Charles Leigh, Mr. P. J. Anderson, Mr. A. P. Hunt, and Mr. Peter Bennett, for the cordial way in which they facilitated my work.

[15] Smithsonian Miscellaneous Collections, Nos. 850, 1170, and 1440.

XXIII. On a Specimen of *Osteocella septentrionalis* (Gray).

By Sydney J. Hickson, F.R.S.

Professor of Zoology in the University of Manchester.

Received and read May 9th, 1911.

In January of this year Professor Bell asked me to examine "some pieces of *Osteocella* with the polyps on," that had been sent to him by Rev. J. H. Keen. My memory being at fault, I referred to the literature of *Pennatulida* and found that Gray's genus *Osteocella* founded on some specimens of the axes of *Pennatulids* had almost been forgotten, the specimens, when referred to, being assigned without much reason by different authors to other and better known genera.

The "pieces," unfortunately, do not make up the whole of this magnificent specimen, but represent different regions cut out in lengths of from 100—200 mm.

Fortunately, however, the pieces are very well preserved, and it is possible to study nearly all the important features that the species exhibits.

The following particulars were supplied by Rev. J. H. Keen:—The specimen was taken off Lucy Island, seven miles S.W. of Metlakatla, British Columbia, in about 30 fathoms, December 15th, 1910.

"It came up on a halibut hook with a dogfish. The dogfish, on finding itself hooked, had rushed round and round the pennatulid, which was completely tied up in the line. It was alive when procured and writhed like a worm.

June 16th, 1911.

The *colour* was pale pink, foot and stem paler than the part bearing the polyps.

The polyps emitted a bright blue-green colour on being irritated. The whole object was covered with a thick slime.

The total length was 6 feet 10 inches, *i.e.*, about two metres. The circumference of the middle region of the rachis was about $1\frac{7}{8}$ inch = 47 mm., of the upper part of the stalk $1\frac{3}{8}$ inch = 35 mm., and of the bulbous enlargement of the stalk about $2\frac{7}{8}$ inch = 74 mm."

The axis. A complete description of the axis cannot be given, owing to the imperfect condition of the specimen, but I have compared some fragments of it with the complete type specimen of *Osteocella septentrionalis* in the British Museum.

The length of the type specimen is 64 inches, and the greatest diameter is 5 mm. The diameter of the axis in the middle region of the stalk of the specimen sent to me is 7 mm. Whether this represents or does not the greatest diameter of the axis when it was complete I cannot say, but judging from this fact, it seems to me probable that the axis of this specimen was longer than that of the type. In structure it resembles the type specimen very closely. It is very hard and in section shows concentric growth rings. Its structure appears to be uniform in the respect that there is no dark central core, similar to that figured by Kölliker (7) in the axis of *Virgularia.*

The comparison of the two axes leaves no reasonable doubt that our specimen belongs to the same genus as the type specimen of *Osteocella septentrionalis* in the British Museum.

The autozooids are arranged in close set pinnæ on both sides of the rachis. There are about 12 autozooids

in each pinna. Each autozooid consists of a retractile portion or anthocodia which is almost completely retracted and a non-retractile basal portion usually called the calyx. The length of the calyx in the fully developed pinnæ is about 4 mm.

Owing to the imperfect condition of the specimen I cannot venture to give a more definite statement of the number of the autozooids in the pinnæ. Many of the pinnæ are broken, and others covered with an adhesive and dirty mucus, so that it requires some care to count the number of the autozooids accurately. I have cleaned two or three unbroken pinnæ from each piece, and have found the number to be constantly twelve, but it is still possible that there may be some variations from this number.

The pinnæ of the two sides alternate, or to use Moss's expression, are arranged "en echelon."

On the dorsal side there is a broad smooth track free from autozooids, on the ventral side there is in some parts of the rachis a narrow smooth track, but at the free extremity the pinnæ of opposite sides overlap ventrally.

The pinnæ of each side, when fully developed, slightly overlap the pinnæ immediately above them, so that the distance between the pinnæ is from 3·5-4 mm. In a portion of the middle of the rachis, 50 mm. in length, I have counted 11 pinnæ.

In the rough sketch of the specimen forwarded to Professor Bell, the rachis occupies approximately two-thirds of the total length, and from this a rough estimate may be made that there were probably about 450 pinnæ on each side.

In each pinna the outlines of the calices can be seen extending to the base, but they are firmly bound together by a web, in which cellular cords and canals can be seen

in sections. In this respect the pinna resembles that of a *Virgularia*. It is very unfortunate that the character of the pinnæ of the lower end of the rachis cannot be determined, as that part of the specimen was not included among the pieces sent to me. There are two reasons, however, for believing that the lower part of the rachis resembles, in general character, that of *Virgularia*.

In one of the pieces, which I take to be the lowest part of the rachis represented, there are three or four zooids on the ventral side of the pinna so immature that they have very much the appearance of siphonozooids.

In the description given by Moss of a specimen which is clearly identical with ours, it is clear that the pinnæ at the lower end of the rachis are, as in *Virgularia*, not fully developed.

There are no spicules either in the autozooids or in any other part of the rachis. The siphonozooids are to be seen on the smooth dorsal track. They appear to be arranged in four or five longitudinal rows on each side of this track, the siphonozooids of each row being situated at distances of 3 or 4 mm. apart. I have been able to discover that there are also small siphonozooids between the pinnæ, as there are in many species of *Virgularia*.

With reference to the siphonozooids, I wish to make one remark before passing on to other characters of this remarkable pennatulid. It is, in my experience, a very difficult matter to be certain that siphonozooids are not present in any part of a pennatulid, unless that part is carefully examined in prepared sections. Some siphonozooids can be easily seen in the unaltered spirit specimen, but others are so retracted that they can only be seen in thin preparations. As an example of this, I may refer to the small siphonozooids that occur on the stalk and bulb of *Umbellula carpenteri* (5). Unless extreme care is taken,

and many series of sections cut, therefore, it is very misleading to found generic or specific characters on the supposed absence of siphonozooids from any one region. I may say, however, that the statement made above concerning the distribution of siphonozooids in this specimen has been confirmed by the examination of sections.

The stalk of the specimen is so imperfect that it is impossible to give a full and accurate account of it. There is one point of interest about it, however, to which reference may be made. Moss (11) refers to and figures four "lines of pores on the stalk" in his specimen. These "pores" can be easily seen on some parts of the stalk, but it is perfectly clear from an examination of a stained section that they are not simple pores, but siphonozooids. I cannot find them on the bulbous enlargement of the stalk, only on the narrow part, but I have not made an exhaustive examination of the bulb. In the stalk there are a few scattered calcareous spicules, oblong in shape, and about ·08 × ·03 mm. in size. (*Fig.* 3.)

The most remarkable, and, to my mind, characteristic feature of our specimen, has yet to be described. As compared with many other pennatulids, it is extremely fleshy. This fleshiness is due to a great increase in the mesoglœa, with its penetrating canals and cell cords, on the ventral side of the ventral longitudinal canal. (The terms "dorsal" and "ventral" are applied as suggested by Jungersen (6), and are the exact opposite of the same terms as used by Kölliker (8).)

The significance of this may be seen in the diagrams (*Figs.* 1 and 2), which show a transverse section of the rachis of *Osteocella* and of *Virgularia.*

In this mass of flesh, and opening into the main ventral canal, there may be seen a row of long tubes. (R.)

These "radial" canals are on the ventral side of the rachis, and do not therefore correspond in position with

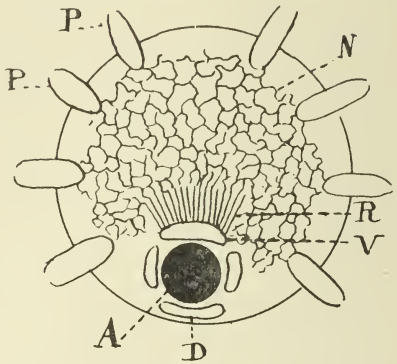

Fig. 1.

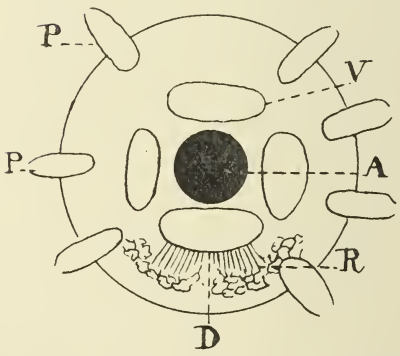

Fig. 2.

Figs. 1 and 2.—Diagrams of a transverse section through the rachis of *Osteocella* (*Fig.* 1) and *Virgularia* (*Fig.* 2). *A*, the axis. *D*, the dorsal, longitudinal vessel. *N*, the network of canals in the mesoglœa. *P*, the position of the pinnæ as seen in a transverse section. *R*, the radial vessels opening into the ventral canal in *Osteocella*, and into the dorsal canal in *Virgularia*. *V*, the ventral longitudinal canal.

the "radial" canals of *Virgularia* and *Halisceptrum*, which are on the dorsal side of the rachis.

One effect of this great development of fleshy substance on the ventral side is that the axis is not central in position, but dorsal to the main mass of the soft structures of which the rachis is composed.

With reference to this character, a passage in the paper by Moss is noteworthy. He says "lateral ridge-like processes bearing the polypes exist only on one side of the central axis, in short, to borrow a term from its fossil relatives, the *Graptolitida*, it is monoprionian." We may suppose that Moss's specimen being badly preserved was considerably compressed laterally, and the axis, consequently, being seen to project on one side, gave the *pennatulid* a monoprionian appearance.

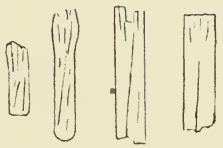

Fig. 3.—The spicules in the superficial layers of the stalk of *Osteocella*. × 150 diams.

In a note on the structure of this species by Dr. Blake, which follows Stearns' description (15) there is also a crude figure illustrating this point.

In a description of a new species of *pennatulid* which he named *Pavonaria dofleini*, Moroff (10) gave a figure of a transverse section of the rachis, which is very similar in this respect to that of *Osteocella*. In writing of the rachis he remarks, "Seine ventrale Seite ist frei und gewölbt." In this respect, and, I believe, in others, Moroff's species is closely related to *Osteocella*.

The specimen is a female, clusters of eggs occurring on the mesenteries in the cœlenteric canals (Solenia) of the rachis. As in *Virgularia* there are no ova in the

pinnæ, but large ova are found in the autozooids of fully formed pinnæ up to the base of the uppermost "piece" in my series. Being unwilling to dissect the extreme tip of this specimen, I cannot say whether or not they extend the whole length of the rachis, but it is a fact that the autozooids at a distance of 100 mm. from the top are "fertile."

This is an important point in determining the systematic position of our specimen, because in *Virgularia* the reproductive organs are "confined to the lower part of the rachis, and only occur in that part of it in which the polypes are either absent or very immature (Marshall)"; whereas in *Pavonaria*, "Geschlechtsorgane in den Blättern mit entwickelten Polypen" (Kölliker (8)).

The generic name *Osteocella* is first mentioned in Gray's Catalogue of Sea-pens published in 1870. The type specimen consisted of the axis only, and was sent to the British Museum by Mr. G. Clifton.

In 1872, Gray (3) published a further description of this axis of *O. cliftoni*, and added another species based on an axis sent by the Hudson Bay Company to which the name *O. septentrionalis* was given.

The locality of the *O. cliftoni* was, in the first instance, given as "probably Australia," but on the second communication it was stated more definitely, but without any assigned reason, to be W. Australia.

In an additional note (4) on *Osteocella septentrionalis*, also published in 1872, Dr. Gray says that he was informed by Dr. Günther that it was often found in Buzzard inlet, near New Westminster, British Columbia. "Buzzard" is probably a mistake, as the only inlet in this region marked in the maps is named "Burrard." The axis of *O. cliftoni* was 11 inches in length, and that of *O. septentrionalis* 64 inches in length. Verrill expressed

the opinion that it was the axis of a " *Virgularia* or some similar creature."

In 1873 Moss described a *Virgularian actinozoon* from Burrard's inlet, Frazer River, British Columbia, which he considered might be identical with Gray's *Osteocella septentrionalis.* He gave a good description of the specimen and some figures which leave no doubt that his species is the same as that now described. Moss's specimen was 8ft. 6in. in length.

In August of the same year Stearns (**13**) described an *Alcyonoid polyp* from Barracuda inlet, B. Columbia, with an axis very similar to that described by Gray as *Osteocella.*

Stearns placed his specimen in Cuvier's genus *Pavonaria*, but subsequently gave it the new generic name *Verrillia.* In a later paper (1882), however, he refers to a note by Professor Verrill, who considered the specimen he examined to be allied to *Halipteris christii*, and consequently transfers it to the genus *Halipteris* with the specific name *H. blakei.*

In more modern times the genus *Osteocella* has been referred to by Jungersen, Balss, and Nutting. Nutting (**12**) accepting Verrill's view that the generic name *Pavonaria* is a synonym of *Balticina* (Gray) considers that the specimens preserved in the Stanford University Museum in California, and labelled *Verrillia blakei*, cannot be separated from the species *Balticina* (*i.e.*, *Pavonaria*) *finmarchica* (Sars).

If we are justified in assuming that all these specimens from the west coast of N. America belong to the genus, we see that the generic name has shifted as follows :— *Osteocella* (1870), *Virgularia* (1872), *Pavonaria* (1873), *Verrillia* (1873), *Halipteris* (1882), *Balticina* (1909).

All these changes in generic names have been made without any description of the structure of the *Pennatulid* as a whole that will justify its definite inclusion in any one of the genera whose characters have been well established.

Nuttall, it is true, says that Stearns' original description of *Verrillia blakei* was a "very complete one," but Jungersen remarks "the descriptions are so imperfect that it cannot be decided with certainty which of those closely allied genera (*i.e.*, *Pavonaria* and *Halipteris*) is in question."

The specimen that has been sent to me does not belong to the genus *Pavonaria*, because there are no spicules in the pinnæ, nor can it be placed in the genus *Halipteris* for the same reason, and also because the autozooids are bound together to form pinnæ.

I cannot agree with Moroff that in a case of this kind the presence or absence of spicules in the pinnæ is a matter of no moment.

Nutting, referring to the specimens labelled *Verrillia blakei* in the Stanford University Museum, remarks "these specimens are preserved in glycerin, and the spicules seem to have largely been dissolved"; but the spicules in the tentacles of *Pavonaria finmarchica* are 1·8—2·1 mm. in length, and it is difficult to understand why spicules of this size should be dissolved entirely in glycerin.

In 1902, Moroff described two new species of *Pennatulids* that were obtained by Dr. Doflein from the coast of California. One of these, to which he gives the name *Pavonaria dofleini*, was 1250 mm. in length, the other *P. californica* 700 mm. in length. It is very difficult to understand Moroff's reason for including these specimens in the genus *Pavonaria*.

Among the characters given by Kölliker (8) of the genus *Pavonaria* are these "Lange, starke Seefedern mit kurzem, dickem Stiele, und dicken, niedrigen Blättern deren Rand nur undeutlich in Kelche geschieden ist." From the figures given the calices are quite clearly differentiated in Moroff's species.

"Radiäre Kanäle fehlen." In *P. dofleini*, according to Moroff, radial canals are present, but no statement is made as to their position in relation to the dorsal or ventral side of the rachis ; and they are not shown in the figures.

"Kalkkörper von typischer Nadelgestalt in der Hauptstammen der Tentakeln." In *P. dofleini* there are no spicules in the tentacles. Moroff's statement that spicules are abundant in the wall of the pinnæ and in the whole stock, however, proves that there is an important difference between his specimens and the specimen from Metlakatla. But for this difference I should be inclined to believe that *Pavonaria dofleini* is a moderately large specimen of *Osteocella septentrionalis* and that *P. californica* is a younger form of the same species.

It is a great pity that Moroff gives no statement or figure of the size or shape of the spicules in the pinnæ.

If *P. dofleini* is a distinct species, it is a very remarkable fact that two gigantic sea-pens presenting the same exceptional structural features should occur on the same coast.

Nutting's species *Balticina pacifica* appears to me to be quite distinct, but the specimens he refers to the species *Balticina blakei* (? *B. finmarchica*) are probably identical with *Osteocella septentrionalis*.

I may remark, at this point, that I object very strongly to the change of name from *Pavonaria* to *Balticina* and refuse to accept it. The genus *Pavonaria*

was described at length by Kölliker and is well understood. To abandon it now in favour of an older and forgotten generic name will only lead this branch of science into confusion.

After full consideration of all the characters of the specimen I have come to the conclusion that it cannot justifiably be assigned to any of the well-known genera of *Pennatulids* but represents the type species of a distinct genus.

Its closest affinities are undoubtedly with the genus *Virgularia*, but it differs from *Virgularia* in the great expanse of fleshy substance on the ventral side of the rachis, in the distribution of the sexual organs throughout the rachis, and in the position of the radial canals on the ventral side.

It differs from *Pavonaria* in the absence of spicules in the autozooids and rachis, in the absence of genital organs in the pinnæ and in the presence of radial canals.

The account given by Kölliker (8) of the structure of the rachis of *Pavonaria finmarchica* is quite sufficient to prove that this, the type species of *Pavonaria*, does not belong to the same genus as *Osteocella*.

But if it belongs to a distinct genus, to what genus should it be correctly assigned?

In a discussion between Stearns and Gray in 1874 on the question of priority, the former asserted that "No description sufficiently accurate to be worthy of consideration can be made of the axial rods or bones of this class of animal forms." Notwithstanding this assertion, however, there can be no doubt from an examination of the axis in the British Museum, described by Gray under the name *Osteocella septentrionalis*, that it is the axis of a pennatulid belonging to the same genus, and judging from

the locality and size, probably to the same species as the specimen sent to me.

Consequently, according to the International Commission's Art. 27, which says, "The Law of Priority obtains, and consequently the oldest available name is retained: when any part of an animal is named before the animal itself"—the correct name of the specimen is *Osteocella septentrionalis.*

My summarised description of *Osteocella septentrionalis*, Gray, then is as follows :—

Large Pennatulids attaining to a total length of over two metres.

Rachis at least twice the length of the stalk.

Pinnæ arranged "en echelon" on the sides of the rachis, each composed of about twelve autozooids, bound together by fleshy webs between their non-contractile calicular parts. Each pinna slightly overlaps the pinna immediately above it. There are no spicules in the tentacles of the autozooids, nor in the pinnæ nor in the other parts of the rachis.

Siphonozooids between the pinnæ, and in four or five longitudinal rows on each side of the dorsal smooth track. Siphonozooids also present in three or four rows on the sides of the upper part of the stalk.

Stalk smooth, with a large bulbous swelling, with scattered oblong spicules, about ·08 × ·03 mm. in size.

Axis of great length, very hard, round in section, and exhibiting concentric rings of growth.

The genus shows a great development of fleshy tissue on the ventral side of the ventral longitudinal canal. This tissue is perforated by well marked radial canals. Sexual organs situated in the cœlenteric canals of the autozooids in the rachis but not in the pinnæ. They are found in this position throughout the whole length of the rachis.

The best figures and description hitherto published are by Moss (**11**).

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XXIV. An Account of some Remarkable Steel Crystals, along with some Notes on the Crystalline Structure of Steel.

By Ernest F. Lange,
M.I.Mech.E., Assoc.M.Inst.C.E., M.I. & S.Inst., F.C.S.

Read May 9th, 1911. Received for publication June 20th, 1911.

The natural occurrence of freely developed crystalline forms in steel has been so rarely observed, that, with the kind permission of Messrs. Vickers Ltd., I have pleasure in bringing before your notice the most perfect example of the free development of a large group of steel crystals with which I am acquainted. These crystals were discovered by Colonel T. E. Vickers, C.B., who happened to be examining the pipe in a large rising head which had just been removed from a heavy steel propeller-boss casting, and who noticed their presence in the upper part of the cavity. Impressed with the metallurgical importance of the find, he had the mass of metal carefully sawn in two, and the hollow portion freed from the surrounding mass, and the cavity thus revealed photographed, as a permanent record of its appearance. This photograph is just half the natural size, and, as you will see from the copy before you, it shows the cavity incrusted all over with "pine-tree" shaped crystals in various stages of development. The vertical crystals have formed with remarkably little interference, some being separate, and in a few cases reaching the remarkable length of 14 or 15 inches. (See *Fig.* 1, *Plate I.*).

August 21st, 1911

I believe I am right in saying that this photograph is unique of its kind; I have searched in every likely publication for a similar illustration, but in vain; I have only been able to find an illustration of a single steel crystal, a very fine "pine-tree" specimen, similar to one of the largest in the present case, which belongs to Professor D. Tschernoff, of St. Petersburg, and which was taken from a cavity in the rising head of a 160 ton ingot of soft open-hearth steel. The reason why such freely developed steel crystals have been but seldom noted, in spite of the fact that steel is a crystalline body, is that they can only form under very exceptional circumstances. The fact that they have only been noted in conjunction with large castings or ingots indicates that very slow cooling is necessary to their formation, and the position in which they have been found indicates that they have formed during the slow descent of liquid steel from the upper to the interior portion of a mass of steel during the contraction caused by the cooling. In fact the formation of the cavity has given room for the free development of the crystals starting from a comparatively few points as may be expected in very slow cooling. The position of the crystals in the rising head, and close, therefore, to the segregated area, would doubtless be reflected in their composition, and this is shown in the present case by the two analyses that Messrs. Vickers Ltd. have supplied me with. The first analysis was made from drillings taken from an actual crystal, and showed the following results:—

Analysis of an actual Crystal.

Combined Carbon	·43 %
Silicon	·191 ,,
Manganese	1·05 ,,
Sulphur	·101 ,,
Phosphorus	·098 ,,

A second analysis was now taken of the steel at a point about 1 inch from the cavity. It was not possible to get clean drillings from any point further away from the "pine-tree" crystal area than this owing to the metal having been all machined away close to the cavity.

Analysis outside of the "Pine-Tree" Crystal area.

Combined Carbon	·345 %
Silicon	·16 „
Manganese	1·02 „
Sulphur	·08 „
Phosphorus	·076 „

I have not the composition of the charge from which the propeller-boss casting was made, but am informed that the bulk of the metal in that particular casting would not contain more than ·05 to ·06 of either sulphur or phosphorus. There has, therefore, been considerable segregation in the area where the "pine-tree" crystal growth has occurred.

A description of the external form of these "pine-tree" crystals is not an easy matter. The "pine-tree" crystal form was not produced in the laboratory work of Dr. J. E. Stead and Messrs. Osmond and Cartaud, although the individual component forms were. As early pointed out by Tschernoff, Sorby, Andrews and others, the crystal form of iron is regular.

Osmond says that the crystal belonging to Professor Tschernoff resembles a crystal of alum. This description applies equally to those under consideration. Osmond refers to the crystalline structures which characterise iron in industrial products as resembling skeletons of octahedrons united along the axis. He further refers to some crystallites of the cubic system obtained from a solution of chrome alum with orthogonal branches and

octahedral envelopes, which he says recall exactly the beautiful steel crystal of Professor Tschernoff.

The late Dr. Hermann Wedding also refers to the "skeleton" structure saying that "in commercially pure "iron the crystals show the form of an incomplete regular "octahedron, that is to say, the external form corresponds "to the octahedron, but the mass of the crystal is not filled "up, but is replaced by beams (*Balken*) which run in a "direction parallel to the octahedron axes, and conse- "quently correspond to the position of the belonging cube "faces. Such "beams" have, in the rule, side "beams" "standing at right angles to the main beams, these also "again have often perpendicular beams of the third "degree, so that the whole is only the skeleton of an "octahedron, and has the external form of a pine-tree, or "as the mineralogist would say, receives a knitted or "net-work (*gestrickte*) form."

Messrs. Vickers Ltd. kindly placed at my disposal for microscopical examination the end portion of one of the shorter crystals. This was cut into two halves longitudinally, bisecting two opposite angles. The two cut surfaces of one of these halves were then ground up to an exact right angle with each other, and then polished and etched with picric acid.

On examining the section with the unaided eye, a very striking pattern of remarkable geometrical symmetry was revealed, particularly on allowing the light to fall on the section from an angle. This I photographed direct, and then enlarged to twice the linear dimensions. This photograph is faithfully reproduced in *Fig.* 2, *Plate II.*, without the smallest retouching in any way, and speaks for itself as regards the regularity of the pattern. A line corresponding to the main longitudinal axis is plainly seen, and this is intersected at right angles by a series of parallel lines

coincident with or parallel to the "terraces" on the exterior so plainly to be seen in *Plate I.* These parallel lines are intersected again at right angles by evenly spaced short lines, which can be clearly counted in the photograph.

To the metallographist the photograph suggests the appearance of rectangular lines of lighter coloured ferrite upon a darker pearlite background, but this is not the case. The bulk of the ground, it is true, is pearlite, but the pattern, as shown, is produced by lighter coloured lines of pearlite on the dark ground, apparently brought out by slight overetching. On turning the section so as to allow the light to catch the bright uncoloured ferrite, it will be seen that this has arranged itself with almost as obvious a general symmetry along the boundaries of the network pattern as shown by the photograph. Under a low power of magnification the ferrite appears as disconnected uneven lines and rectangles, but the pattern made by the ferrite as a whole is, as before said, unmistakeably in perfect agreement and relation with the pattern in the pearlite itself.

Under a high power there is little to distinguish the structure, as revealed in so limited a field, from that of an ordinary unannealed but slowly cooled casting.

The structure also showed a great deal of the dove-coloured sulphide of manganese, which was not surprising in view of the analysis. This sulphide of manganese mostly occurred, as is commonly the case, in the centre of the ferrite lines and patches.

There is not the smallest doubt that the pattern revealed by the etching stands in intimate relationship to, and is indicative of, the interior crystalline structure of the steel "pine-tree" crystal, and affords valuable evidence of crystallization in a regular system.

As remarked at the beginning of this communication, the formation of crystals of iron or steel having truly geometrical boundaries is of rare occurrence. The question might then be asked in what manner is the crystallization of steel revealed in its solidification from the liquid state in the ordinary way, where the mass of the steel prevents the formation of freely developed crystal forms owing to the mutual interference of the crystal growths.

Tschernoff assumes that iron solidifies not in approximately parallel layers, but by the growth of "pine-tree" crystals (*Revue Universelle*, ser. 2, vol. 7, 1880). I have myself seen the etched surface of the whole longitudinal section of a large steel ingot, the structure of which at some distance suggested the appearance of interlacing branches. However this may be, this tendency is not independent of other physical circumstances, for it has been observed that the interior of "bled" ingots, that is to say, ingots from which the interior has escaped after partial solidification, is always smooth. We also know that iron or steel, like many other metals, when solidifying in a mould through the walls of which the heat is rapidly conducted away assumes a strongly columnar structure.

Many years ago Müller, in Germany, conducted a series of experiments on steel ingots to determine the stages of the growth of blowholes, and to do this he poured out the interior of partly frozen ingots at various stages of solidification. He invariably found the interior surfaces smooth and without any surface protrusion of crystal growths (*Iron*, January 5th and September 14th, 1883). It would appear, therefore, that the solidification of steel in the ordinary way proceeds in smooth parallel layers in spite of a strong columnar structure, or, at all events, the growth of the crystals keeps absolute pace with the solidification of the steel. You will notice this columnar

structure well marked in the ingot section on the table. (See *Fig.* 3, *Plate II.*).

Professor Howe, in America, tried to obtain side light on this matter by pouring out the interior of freezing ice ingots, but found the walls to be perfectly smooth. He also experimented with blocks of slag in the same way, but again found the sides of the emptied cavity smooth and free from crystalline markings, although the solidified portion had a strongly columnar structure.

Referring to the very strongly marked columnar structure of solidified steel at right angles to the cooling surfaces, you will see, in the case of the ingot section shown on the table, that the section is not wholly columnar; the section is nine inches square and the largest columns about $3\frac{1}{2}''$ long, and an area of about $3''$ square in the centre is confused or granular. The steel has the following analysis:—

Combined Carbon	...	...	=	·16 %
Silicon	...	...	=	·046 ,,
Manganese	...	...	=	·55 ,,
Phosphorus	...	...	=	·049 ,,
Sulphur	...	...	=	·032 ,,
Nickel	...	...	=	5·35 ,,

The composition of this steel indicates a high melting point; there is, therefore, less direct transition from the liquid to the solid state, and the confused area would represent the disturbance of the sluggish core sinking more or less towards the lower part of the ingot. The segregation in the centre of the section shows that the same is only just clear of the bottom of the pipe, which, as is usual, had formed in the top of the ingot.

It is quite obvious that there is a similarity of cause and effect between the phenomena of crystallization as noted in the "pine-tree" crystal growth and that of the

columnar formation. In both cases there is uniformity of orientation along a main long axis. Rhead believes that the difference of conductivity between the solid growth and the liquid mass is responsible for the uniformity of orientation in the former case, and certainly the chilling effect of the mould produces the same result in the latter case.

You will notice that the columnar crystals do not appear to have much cohesion, and, in fact, the surface cracks often observed in rolling or forging ingots usually pass between these crystals.

It will be observed that there is no regularity in the size or formation of these columns; the lateral growths are irregular, so that the columns interrupt each other in their growth at different distances from their main axis, and there is also no relation in the angles of the lateral growths of the neighbouring crystals. In fact, the crystal growths follow the ordinary law of chemical solutions. When a crystal starts from a point it grows in all directions until interfered with by the growth of adjacent crystals. For the same reason, slow cooling results in large crystals, and quick cooling in small crystals. The columnar structure is due to the rapid absorption of heat by the sides of the mould, tending to make the steel crystallise in long prisms at right angles to the surface, although naturally the crystal grains tend to an equiaxed formation. As the walls thicken, and the flow of heat lessens, the prismatic tendency weakens, and there is a sudden change from the prismatic to the equiaxed formation. There is also, as before remarked upon, the interstratal action of the liquid core.

These remarks as to the formation of a columnar structure during solidification hold good for any steel casting whether in an iron or a sand mould; and at all

junctions in rectangular forms, the meeting lines of the columnar formation constitute a great weakness to the casting, which is removed by annealing when the casting is reheated through a certain range of temperature, and kept at a certain heat for a certain length of time, according to the composition of the steel and the nature of the mechanical properties required from the material of the casting.

The influence of heat treatment upon the crystalline structure of steel was perhaps most generally made known through the researches of Brinell, and the publicity given to his experiments by the exhibit of the Fagersta Company in the Swedish section of the Paris Exhibition of 1900. A copy of Brinell's chart, showing a graphical representation of the changes of fracture and carbon in steel containing 0·75 per cent. of carbon while heating and cooling, and dated Fagersta, 1898, lies before you. In this chart Brinell shows that however coarsely crystalline the texture of the steel might be, on heating the same to the temperature which he calls the "moderate hardening heat *IV*," corresponding to the temperature at which the carbon of the steel suddenly passes from the carbide or cement state into the hardening state, the steel assumes the finest structure that it is capable of receiving. (Stead referring to this particular chart assumes *IV* to be about 750°C.). If the steel is heated beyond the temperature *IV*, the crystals begin to grow and get the coarser the higher the heat attained. The structure can, however, be restored to the desired fineness by allowing the steel to cool below a temperature denoted as the "annealing heat *V*," and then to be re-heated to *IV*. The annealing heat *V* denotes the temperature at which the carbon in steel containing hardening carbon has, during cooling as well as heating, the greatest tendency to pass into cement

carbon ; this is the temperature of the well-known phenomenon of recalescence. Brinell does not state the actual temperatures for *V* and *W* in this case, but Howe states in reference to the same that Brinell's temperature *V* appears to be Ar_1 (690°C.), and his *W* appears to be $Ac_{2,3}$, and therefore to vary with the percentage of carbon as shown in Roberts-Austen's diagram (*vide* Fifth Report of the Alloys Research Committee, 1899).

If steel is heated to a temperature above *W*, and allowed to cool slowly without work, the crystal grains continue to grow until the temperature *V* is reached, beyond which there is no further growth. Now the action of forging or rolling strongly opposes crystallization in the case of both iron and steel. Like agitation of salts crystallising from an aqueous solution, it appears to arrest the development of crystalline structure. It follows from this that all forged work should be finished as near to the temperature *V* as possible, so as to prevent the formation of a coarse structure during subsequent undisturbed cooling, and it also follows from this that all forgings which are not finished at one heat should have their structure refined by re-heating the whole forging to a suitable temperature as a last operation, in order to destroy any coarsely crystalline structure produced in the parts first finished by reason of their proximity to the portion last heated up for finishing.

The experiments of Stead and Heyn show that, for breaking up coarsely crystalline structures in very mild steels, the refining temperature should be about 900°C., or beyond the highest critical point for iron, and an examination of Brinell's later experiments on the heat-treatment of steels of varying carbon contents would appear to make this hold good for carbon steels also, if

the best refining temperature is held to be that which produces the best mechanical tests.

In the experiments of Osmond and Cartaud, alluded to in the beginning of this paper, the investigators were able to prove that the three modifications of iron*

*Adopting the nomenclature of Osmond (vide *Transformations du Fer et du Carbone dans les Fers, les Aciers et les Fontes blanches;* Baudoin, Paris, 1888), as was done by Sir W. Roberts-Austen in the Fifth Report of the Alloys Research Committee, 1899, to the modifications of iron that take place at certain temperature ranges, the term "gamma" iron is applied to iron above its highest critical point, the term "beta" iron to iron between this point and its magnetic change point, and the term "alpha" iron to iron between this point and the temperature of recalescence. The points observed on heating, Osmond marked "*c*" (*chauffage*), and the points on cooling "*r*" (*refroidissement*).

The following figures are recent estimations by Professor Arnold, of Sheffield University (vide Lecture "On a Fourth Recalescence in Steel," 1910):—

	Critical Points in Cooling Curves.		
	Ar_3	Ar_2	Ar_1
Absolutely pure Iron	854°C.	750°C.	None.
·22 % Carbon Steel	819°C.	764°—747°C.	696°C.
·38 %	726°C.		696°C.
·63 %	710°C.		695°C.

A heating curve with ·20 carbon steel showed the following ranges:—

718°—729°C. Ac_1, transformation of pearlite into hardenite (cement into hardening carbon).
735°C. End of "alpha" range of temperature.
738°—753°C. Ac_2, magnetic change point.
756°C. Beginning of "beta" range of temperature.
840°C. Maximum of Ac_3.
850°C. Beginning of "gamma" range of temperature.

The recalescence data of a pure saturated carbon steel (c.c. = ·89 %) were determined by Arnold and McWilliam as follows (vide *Journal of Iron and Steel Institute*, 1905, II.):—

On heating.....$Ac_{1, 2, 3}$, maximum at 720°C. Range 710°C. to 730°C.
On cooling.....$Ar_{1, 2, 3}$, maximum at 675°C. Range 690°C. to 660°C.

Ac_1, Ar_1 have to do with the carbon change points, Ac_2, Ar_2 and Ac_3, Ar_3 with the iron change points, but, as seen above, the carbon is the dominating influence, and with increasing carbon contents all these change points gradually merge into the one corresponding to the carbon change point only.

crystallised in the regular system. They reduced ferric chloride in a current of hydrogen at temperatures corresponding to the different allotropic forms, and then examined under the microscope the crystals thus obtained. The results showed that "gamma"-iron occurs in all combinations of the cube with the octahedron, and that "beta"- and "alpha"-iron crystallise in cubes, and are isomorphous. From what has already been said as to the formation of the crystalline structure in solidifying steel under mutual interference of the crystal growths, we shall be prepared to find, on examining a polished and etched section of normally cooled mild steel under the microscope, granules possessing definite boundaries and without definite geometrical form. We do, in fact, find that the structure of such steel reveals a pattern of irregular polyhedra. These polyhedrons are, however, differently affected by the acid and reflect the light differently, thus showing that they each possess a different orientation, and by carrying the etching further it is frequently possible to see the perfect uniformity of orientation of the secondary crystals within each of these polyhedrons. As the boundary lines of these granules bear no relation to the internal structure of the same, they are called allotrimorphic crystals as compared with idiomorphic crystals, in which the external form corresponds to the internal symmetry, as is the case in the "pine-tree" crystals which I have just described.

It is not within the scope of this paper for me to refer to the crystalline structure of cast iron, or to the small but perfectly formed crystal structures frequently found in cast iron cavities. The crystalline structures that occur in iron rich in silicon or manganese have also little direct bearing on our subject.

I may, however, refer to the so-called "furnace-crystals" or "bears" of carbonless, or almost carbonless

iron, which are occasionally found in furnace hearths or slag masses. A good example of this formation was once sent to me by the late Mr. H. A. Hoy, which had been taken from a pocket in the hearth of one of the steel-melting furnaces of the Lancashire and Yorkshire Railway Company at their Horwich works. Prolonged exposure to a temperature not far removed from the melting point had oxidised away the carbon, silicon and manganese, whilst by absorption and concentration, the phosphorus content had increased to a remarkable extent as the following analysis of the sample shows :—

Analysis of Horwich "Furnace-Crystals"?

Combined Carbon	nil %
Silicon	trace „
Manganese	nil „
Sulphur	·02 „
Phosphorus	·36 „
Iron	99·62 „

The external appearance of the metal resembled a mass of crystal surfaces, mostly pentagonal, with smooth flat faces, some being of considerable size. These might, at first sight, appear to furnish proof of a definite form of crystallization, but this is not actually the case. Their formation was due to the constant growth of the granules under the combined influence of time and heat; and the effects of expansion and contraction upon groups of such granules in their plastic condition produced the forms of pentagonal dodecahedra such as are produced by the compression of plastic spheroids. This would be helped by the liquation of the more fusible phosphide of iron between their cleavage planes. On breaking up the mass when cold the lines of fracture would be along the brittle

films of the phosphide, thus clearly revealing the formation produced under the above circumstances.

The examination under the microscope of the microstructure of steel sections after polishing and etching, supplies other links of evidence of crystallization in a regular system. The microstructure of an unannealed steel casting in this way shows a pattern suggestive of trellis-work, the white (ferrite) lines forming angular figures. On examining the case-hardened portion of a mild steel bar, we shall find the excess of cementite showing as a series of fine straight lines forming frequently angles of 90 and 45 and occasionally angles of 60 degrees. Sections of quenched carbon steels show the well-known structure of martensite, which has the appearance of being built up of a system of needles running parallel to the sides of an isosceles triangle. On quenching a saturated steel from a high temperature, we obtain a structure of martensite and austenite exhibiting a most marked geometrical pattern of bands crossing at angles of 90 and 45 degrees. Stead has drawn attention to the similarity between the structure of overheated steel and that of unworked and untreated steel that has cooled normally from the liquid state.

The internal crystalline structure of iron and steel has been the subject of a great amount of highly skilled investigation in recent years. The early studies and experiments of Tschernoff, Osmond, Sorby, Le Chatelier and Brinell, paved the way for the later researches of Stead, Roberts-Austen, Sauveur, Roozeboom, Arnold, Martens, Heyn, Carpenter, and others that have lifted the veil that once shrouded the complex phenomena of iron and steel, and have brought their mysteries within the scope of definite laws.

The demand for greater strength of structures, under the ever increasing stress of modern requirements, has also called into being the study and use of special alloy steels, and the same methods of organised scientific research that have been applied to the ferrous metals are now-a-days being employed to increase the efficiency and reliability of the non-ferrous metals with immense benefits to all the industries concerned.

Fig. 1.

Steel "Pine-tree" Crystals,
found in cavity in riser of large steel casting.
About $\frac{1}{5}$th actual size.

Reproduced by kind permission of Messrs. Vickers Ltd.

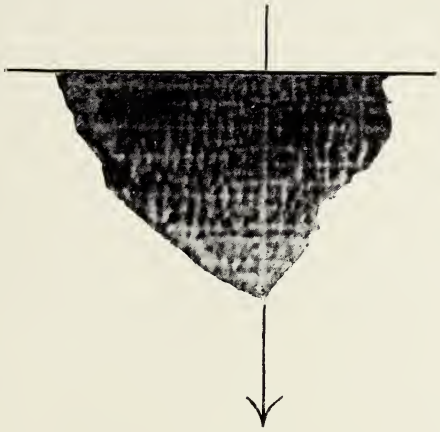

Fig. 2.

Longitudinal section of end of steel "pine-tree" crystal, etched in picric acid and photographed by reflected light.

Scale = 2 : 1.

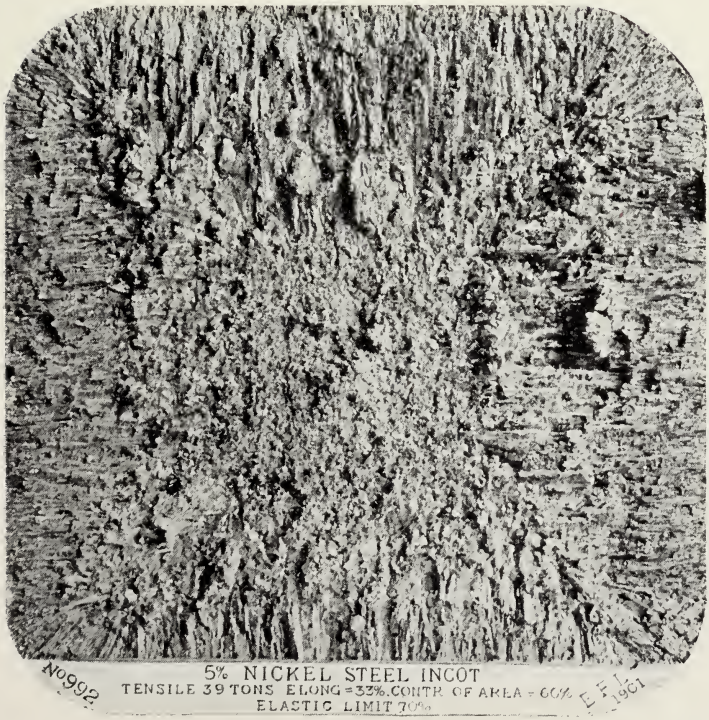

Fig. 3.

Steel ingot broken across to show columnar structure.

PROCEEDINGS

OF

THE MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

General Meeting, October 4th, 1910.

Mr. Francis Jones, M.Sc., F.R.S.E., President, in the Chair.

Mr. E. L. Rhead, F.I.C., Lecturer on Metallurgy at the Municipal School of Technology, Mr. Grafton Elliot Smith, M.A., M.D., F.R.S., Professor of Anatomy in the University of Manchester, and Mr. F. H. Crewe, Assistant Science Master in the Municipal Secondary School, Whitworth Street, were elected ordinary members of the Society.

Ordinary Meeting, October 4th, 1910.

Mr. Francis Jones, M.Sc., F.R.S.E., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the tables. The following were amongst the recent accessions to the Society's Library: "*Oxide of Zinc: its Nature, Properties, and Uses*," by J. C. Smith (12mo., London, 1909), presented by the Trade Papers Publishing Co., Ltd.; "*Adolf Furtwängler*," von P. Wolters (4to., München, 1910), presented

by the K. Academie der Wissenschaften München; "*Astrographic Catalogue 1900·0. Oxford Section Dec. + 24° to + 32°*," vols. 5, 6, by H. H. Turner (Fol., Edinburgh, 1909), presented by the University Observatory of Oxford; "*Flora Capensis*," vol. 5, sect. 1, part 2, by Sir W. T. Thiselton-Dyer (8vo., London, 1910), purchased; "*Historical Sketch* [*of the*] *Museum of Natural History* (*Springfield*), *1859-1909*," by G. P. Johnson (8vo., Springfield, 1910), presented by the Springfield Museum; "*A Retrospect of 50 years' Existence and Work* (*of the Liverpool Geological Society*)," by W. Hewitt (8vo., Liverpool, 1910), presented by the Liverpool Geological Society; "*Supplementary Investigation in 1909 of the Figure of the Earth and Isostasy*," by J. F. Hayford (4to., Washington, 1910), presented by the U.S. Coast and Geodetic Survey; "*Celestial Ejectamenta*," by Dr. H. Wilde (8vo., Oxford, 1910), presented by the author; "*Œuvres Complètes*" de C. Huygens. Tome 12 (4to., La Haye, 1910), presented by the Société Hollandaise des Sciences de Harlem; *A Reconnaissance across the Mackenzie Mountains, etc.*," by J. Keele (8vo., Ottawa, 1910), presented by the Geological Survey of Canada; "*Onderzoekingen in Verband met de Afscheiding van Foezelolie*," door F. Fontein (8vo., Delft, 1909), "*Bijdrage tot de Kennis der Constitutie van het Bixine*," door J. F. B. van Hasselt (8vo., Haarlem, 1910), "*Bijdrage tot de Kennis der Postcenomane Hypoabyssische*," door J. Schmutzer (4to., Amsterdam, 1910), and "*De Bouw van het Siluur van Gotland*," door E. C. N. van Hoepen (4to., Delft, 1910), presented by the Technische Hoogeschool te Delft; "*Three Wisconsin Cushings*," by T. W. Haight (8vo., n. pl., 1910), presented by the Wisconsin History Commission; "*Monograph of the British Nudibranchiate Mollusca*," part 8, by J. Alder, A. Hancock, and Sir C. Elliot (fol., London, 1910), purchased from the Ray Society; "*Bacon is Shakespeare*," by Sir E. Durning-Lawrence (8vo, London, 1910), presented by the author; "*Wheat in India: Its Production, Varieties, and Improvement*," by A. and G. L. C. Howard (8vo., Calcutta, &c., 1909), presented by the Agricultural Research Institute of Pusa; "*Ethnographica in het Museum*

van het Bataviaasch Genootschap," pls. 1-12, door J. W. Teillers (fol., Weltevreden, 1910), presented by the Bataviaasch Genootschap van Kunsten en Wetenschappen; "*Sermones Dominicales*"...*ellátta Szilády Áron*, Köt. 1, 2 (8vo., Budapest, 1910), presented by the Magyar Tudomanyok Akademia; "*The Trade Winds of the Atlantic Ocean*," by M. W. C. Hepworth, J. S. Dines and E. Gold (4to., London, 1910), presented by the Meteorological Office, London; and "*Geologic Atlas of the U.S.*," folios 16-56, 58-69, 71-168 (la. fol., Washington, 1895-1909), presented by the U.S. Geological Survey.

Mr. THOMAS THORP described a method for preventing the tarnishing of silver-on-glass parabolic mirrors, which he had found very successful. The method was, briefly, as follows:—

The mirror was carefully levelled on a turntable, and its axis of rotation made coincident with that of the turntable. The whole was then rotated uniformly at the calculated speed required to cause a liquid to assume the same parabolic form as that of the mirror. A 1 % solution of "Schering's" celloidine in amyl acetate (after a lengthy period of settling) was flooded on to the surface of the mirror to a depth of about one-third of a millimetre. This was allowed to dry very slowly when the resultant film was found to have a perfectly even surface of a thickness of about $\frac{1}{300}$ of a millimetre.

On testing the mirror no perceptible loss of definition was observed, and in actual use the performance was satisfactory.

It is absolutely essential for the success of the method that the mirror be quite enclosed, and exposed only to an atmosphere of amyl acetate, so as not to be allowed to dry, for about one hour after the solution has been flooded on, as, without this precaution, a perfectly uniform film cannot be obtained.

Mr. C. E. STROMEYER, M.Inst.C.E., showed a process, now fairly well known to metallurgists, by which the sulphide segregations in steel are reproduced on photographic paper which has been steeped in a weak solution of sulphuric acid and then placed on

the polished surface of the steel specimen. Twenty-seven impressions from samples of steel containing from 0·03 to 0·20 % of sulphur were exhibited, and it was pointed out that the varying intensities of the impressions were no safe guide to the percentage of sulphur present in the steel.

Dr. HENRY WILDE, F.R.S., read a paper entitled **"On the Origin of Cometary Bodies and Saturn's Rings."**

The paper is printed in full in the *Memoirs*.

General Meeting, October 18th, 1910.

The President, Mr. FRANCIS JONES, M.Sc., F.R.S.E., in the Chair.

Mr. ROBERT MCDOUGALL, B.Sc.; Mr. ROBERT COTTON, M.Sc., Demonstrator in Engineering in the University of Manchester, *Westholme, Devonshire Road, Pendleton, Manchester*; Mr. EVAN JENKIN EVANS, B.Sc., Assistant Lecturer and Demonstrator in Physics in the University of Manchester; Mr. JOSEPH MANGAN, M.A., Lecturer in Economic Zoology in the University of Manchester; Miss EDITH MAY KERSHAW, M.Sc., Demonstrator in Botany in the University of Manchester, *Ash Meade, Upper Mill, Yorks.*; Mr. WALTER MEDLEY TATTERSALL, M.Sc., Keeper of the Manchester Museum, *34, Parsonage Road, Withington, Manchester*; Mr. ROBERTO ROSSI, M.Sc., Student, *2, Lime Grove, Oxford Road, Manchester*; Mr. ROBERT BEATTIE, D.Sc., Lecturer in Electrotechnics in the University of Manchester; and Mr. ARTHUR LAPWORTH, D.Sc., Lecturer in Chemistry in the University of Manchester, were elected ordinary members of the Society.

Ordinary Meeting, October 18th, 1910.

The President, Mr. FRANCIS JONES, M.Sc., F.R.S.E., in the Chair.

The thanks of the members were voted to the donors of the books upon the tables.

Professor G. ELLIOT SMITH, M.A., M.D., F.R.S., read a paper entitled, "**The Convolutions of the Brain.**"

It is well known that in a series of mammals belonging to the same family, the cerebral cortex is more extensive in the larger animals; but the increase in the extent of the cortex does not remain proportionate to the bulk of the creature, because the size of the cerebral cortex is determined by the area of the sensory surfaces of the body which are *relatively* smaller in the larger animal.

It is also well known that, in order that the blood vessels may convey to the cortex an abundant supply of nutriment, and, at the same time, cause a minimum amount of mechanical disturbance, the cortex does not increase in thickness to any extent, so that every addition to its bulk is expressed wholly in the expansion of its superficial area. It follows that in passing from the small smooth-brained members of any mammalian family to its larger representatives, the cortex must become folded in order to be packed in the limited area provided by the surface of the brain.

It has now become possible to explain the nature of the factors which determine and guide the process of folding rendered necessary by these known fundamental conditions.

In all mammals special parts of the cortex become cultivated by each of the senses—one area is set apart to act as a receptive and recording apparatus for impressions of sight, another for hearing, another for touch, another for smell, and so on. In the more highly organised mammals other areas become differentiated

around each of these primary sensory territories to elaborate, as it were, the raw material received by the latter, and also to blend the various impressions, visual, auditory, tactile, olfactory, et cetera, of one object into a consciousness of *all* its properties, and to test and appraise their significance in the light of previous stimulations, the effects of which have been stored up in the various cortical areas.

Thus it comes to pass that the cortex is mapped out into a great number of territories, differing in structure and function, and varying in size in different mammals, not only because the sense-organs themselves vary in size and acuteness in different creatures, but also because in different orders and families a sense organ of a given size will have a varying cortical representation. Thus, if one were to take a dog and a baboon with eyes of the same size, the monkey will be found to possess a much larger cortical visual area than the dog.

It is these differences which determine the varied plans of cortical folding and the resulting varieties in the patterns of the convolutions in different mammals.

Folding occurs most often along the boundary line between two areas of different structure and function. The difference in the rate of expansion of two such areas is no doubt the reason for this type of fissure formation—limiting sulci.

In the second place a rapidly growing cortical territory, meeting with obstruction to its expansion on all sides, may become buckled in, and so a furrow develops along its axis (*i.e.*, within its area), instead of at its edges. This second class of furrow is much less frequent than the first class, and may be distinguished as the group of axial sulci.

There is a third variety, which may be called the operculated sulcus, in which one lip projects over a submerged area. Sulci of this type are produced by the submerging of a specialised fringing territory surrounding a main sensory area.

In the fourth place various mechanical factors come into operation to modify the form of furrows formed in one of these three ways, or even to produce new sulci.

By the application of these principles it is possible to interpret the meaning and the mode of formation of most of the furrows which subdivide the higher types of cortex into numerous convolutions.

General Meeting, November 1st, 1910.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

Mr. JAMES S. BROOME, Science Teacher in the Salford Secondary School, and Mr. ATHERTON GREVILLE EWING MATHESON, Mining Engineer, *Guildhall Chambers, 38/40, Lloyd Street, Manchester*, were elected ordinary members of the Society.

Ordinary Meeting, November 1st, 1910.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the tables.

Mr. R. L. TAYLOR, F.C.S., F.I.C., read the following papers, written by Dr. A. N. MELDRUM, and communicated by Prof. H. B. DIXON, F.R.S.:—"**The Development of the Atomic Theory. ii. The various Accounts of the Origin of Dalton's Theory. iii. Newton's Theory and its Influence in the XVIIIth Century.**"

The papers are printed in full in the *Memoirs*.

Ordinary Meeting, November 15th, 1910.

The President, Mr. FRANCIS JONES, M.Sc., F.R.S.E., in the Chair.

The thanks of the members were voted to the donors of the books upon the tables. The following were amongst the recent donations to the Society's Library: "*The Mineral Resources of the Philippine Islands*," [by] W. D. Smith and others (4to., Manila, 1910), presented by the Bureau of Science, Manila; "*Rapporten van de Commissie in Nederlandsch-Indië voor Oudheidkundig Onderzoek op Java en Madoera, 1908*" (4to., Batavia, 1910), presented by the Bataviaasch Genootschap van Kunsten en Wetenschappen; "*Catalogue of Hepaticæ (Anacrogynæ) in the Manchester Museum*," by W. H. Pearson (8vo., Manchester, 1910), and "*The Tomb of Two Brothers*," by M. A. Murray, [with] Reports by Dr. J. Cameron and others (8vo., Manchester, 1910), presented by the Manchester Museum; "*Catalogue of the British Hymenoptera of the Family Chalcididæ*," by C. Morley, (8vo., London, 1910), "*Guide to the British Vertebrates exhibited in the Dept. of Zoology of the British Museum (N.H.)*," (8vo, London, 1910), "*Guide to the Crustacea, Arachnida, Onychophora, and Myriopoda exhibited in the Dept. of Zoology*" (8vo., London, 1910), and "*Memorials of Charles Darwin: A Collection of MSS., etc., to commemorate the Centenary of his Birth, etc.*" 2nd ed. (8vo., London, 1910), presented by the Trustees of the British Museum.

Dr. W. MAKOWER read a paper, written in conjunction with Dr. S. RUSS, entitled, "**Note on Scattering during Radioactive Recoil.**"

The paper is printed in the *Memoirs*.

Mr. D. M. S. WATSON, B.Sc., read a paper entitled "**Upper Liassic Reptilia. Part III.** *Microcleidus* **and On the Genus** *Colymbosaurus*."

The paper will appear in the *Memoirs*.

General Meeting, November 29th, 1910.

The President, Mr. FRANCIS JONES, M.Sc., F.R.S.E., in the Chair.

Dr. HANS GEIGER, of the Manchester University, was elected an ordinary Member of the Society.

Ordinary Meeting, November 29th, 1910.

The President, Mr. FRANCIS JONES, M.Sc., F.R.S.E., in the Chair.

The thanks of the members were voted to the donors of the books upon the tables.

Prof. ALFRED SCHWARTZ read a paper, written in conjunction with Mr. PHILIP KEMP, M.Sc.Tech., entitled, "**Some Physical Properties of Rubber.**"

The paper will appear in the *Memoirs*.

Ordinary Meeting, December 13th, 1910.

The President, Mr. FRANCIS JONES, M.Sc., F.R.S.E., in the Chair.

The thanks of the members were voted to the donors of the books upon the tables. The following were amongst the recent accessions to the Society's Library. "*Southern Hemisphere Surface-Air Circulation*," by W. J. S. Lockyer (fol., London, 1910), presented by the Solar Physics Committee, South Kensington; "*Catalogue of a Collection of Rocks and Minerals from Natal and Zululand, arranged stratigraphically*," by F. H. Hatch (8vo., Pietermaritzburg, 1909), presented by the Natal Museum; and "*Catalogue of the Dante Collection in the Library of*

University College, London," by R. W. Chambers (8vo., Oxford, 1910), presented by the University College Library, London.

Mr. PHILIP KEMP, M.Sc.Tech., showed experimentally that pure rubber strip which had not been previously extended expanded on the application to it of heat, whereas rubber, which had previously been stretched, on being warmed contracted slightly before it began to expand.

Mr. G. P. VARLEY, M.Sc. (Vict.), exhibited a specimen of Pavonazzo marble from Carrara containing black veins, which, on examination, were found to be crystallised hæmatite, and not graphite as was supposed.

Miss MARGARET C. MARCH, B.Sc., read a paper, communicated by Dr. G. HICKLING, entitled, "**A Preliminary Note on the Effect of Environment on** *Unio pictorum*, *U. tumidus*, **and** *Anodonta cygnea*."

The paper will be printed in the *Memoirs*.

Mr. D. M. S. WATSON, M.Sc., read a paper entitled "**Notes on some British Mesozoic Crocodiles**."

The paper will be printed in the *Memoirs*.

Professor F. E. WEISS, D.Sc., F.L.S., communicated a note "**On** *Sigillaria* **and** *Stigmariopsis*." The author exhibited some specimens of axes of *Sigillaria* associated with Stigmarian bark. From the repeated occurrence of these specimens it was suggested that they represented the base of the aerial or the subterranean axes of *Sigillaria*, probably of the Eusigillaria type.

The secondary wood was more copiously developed than is general in the aerial axes. The primary wood was of Sigillarian type, so that these Stigmarian axes have centripetal primary wood, and their pithcasts would be striated like those described for *Stigmariopsis*.

It was noticed that in some instances small axes were found in contiguity, and apparently in continuity, with the main axes. These smaller axes resemble the ordinary Stigmarian axes very nearly, and do not show the centripetal primary wood of the main axis, but only a few fine tracheids in the pith region.

General Meeting, January 10th, 1911.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

Mr. J. STUART THOMSON, Ph.D. (Bern), Senior Demonstrator in Zoology in the Victoria University of Manchester; Mr. FRANK PLAYFAIR BURT, B.Sc. (Lond.), Assistant Lecturer and Demonstrator in Chemistry in the Victoria University of Manchester, *5, Beaconsfield, Derby Road, Withington*; Mr. FREDERICK RUSSELL LANKSHEAR, B.A., of N.Z. University, Demonstrator in Chemistry in the Victoria University of Manchester; and Mr. ROBERT ROBINSON, D.Sc. (Vict.), Teacher of Chemistry in the Victoria University of Manchester, *Field House, Chesterfield*, were elected ordinary members of the Society.

Ordinary Meeting, January 10th, 1911.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the tables. The following were amongst the recent accessions to the Society's Library: "*Emanuelis Swedenborgii Opera Poetica*" (8vo., Upsaliae, 1910), and "*E. Swedenborg's Investigations in Natural Science, and the basis for his Statements concerning the Functions of the Brain*," by M. Ramström (4to., Uppsala, 1910), presented by the Bibliothèque de l'Université d'Uppsala; "*A Monograph of the British Annelids. Vol. 2, part 2. Polychaeta. Syllidœ to Ariciidæ*," pp. 233-524; pls. 51-56 coloured, and 71-87 uncoloured, by W. C. McIntosh (fol., London, 1910), purchased from the Ray Society; "*Geologic Atlas of the U.S.*," folios 169-173 (la. fol., Washington, 1909-1910), presented by the U.S. Geological Survey; "*Ole*

Römers Adversaria"...Udgivne...ved T. Eibe og K. Meyer (4to., Köbenhavn, 1910), presented by the Kgl. Danske Videnskabernes Selskab; "*Antiquities of Central and South-Eastern Missouri*," by G. Fowke (8vo., Washington, 1910), and "*Chippewa Music*," by F. Densmore (8vo., Washington, 1910), presented by the Bureau of American Ethnology.

Mr. FRANCIS NICHOLSON, F.Z.S., read the following communication.—

"Dr. Adam Bealey and Dr. Dalton."

About sixteen years ago I happened to be in the Society's house when an elderly gentleman, accompanied by another of about his own age, who I understood was a cousin, came in to visit the place where, sixty years earlier, he had received lessons in chemistry from Dr. Dalton.

The visitor introduced himself as Dr. Adam Bealey, and, in the course of conversation, mentioned that he possessed a small bust of Dr. Dalton, which, at my suggestion, he readily agreed to present to the Society. Shortly afterwards the bust was received by the Society, and placed, as desired by Dr. Bealey, in the room on the ground floor on the left as you enter, where it now is on the chimney-piece. A reproduction of a full size photograph of the bust accompanies this note.

As Dr. Bealey's letters to me in connection with this gift are interesting, I have pleasure in presenting them to the Society, if they will accept them. Below are copies of the three letters :—

Filsham Lodge,
Filsham Park,
Jany. 30th, 1894.

DEAR SIR,—

I purpose to send to you the little bust of Mr. Dalton, by Pickford's to-morrow.

I shall be glad if it may be placed in the room in which we formerly sat to read with him.

There is a little book published by Mr. Dalton of which he was personally more proud than of the New System of Chemical Philosophy.

It is an English Grammar. If you have not a copy in the Library and would like this I will send it with pleasure.

Yours faithfully,
ADAM BEALEY.

JOHN DALTON.

St. Leonard's-on-Sea,
Filsham Lodge,
February 15th, 94.

DEAR SIR,—

I was glad to hear from you that the Bust had arrived safely.

I will send the Grammar as soon as I can find it. I was many years in finding a copy.

The bust is not Ivory and will require great care in cleaning. We were assured by Mr. Clare, Dr. Dalton's Executor, that the mould was broken up when twelve were cast. I do not know who had the others.

Dr. Dalton was proud of his little Grammar but knew well the difference between it and his New Chemical Philosophy, by which, as Lord Brougham described it, "he gave numerical laws to Chemistry and raised [it] from an Art to a Science."

Yours faithfully,
ADAM BEALEY.

Filsham Lodge,
St. Leonard's-on-Sea,
Jany. 29th, 1902.

DEAR SIR,—

In reply to your note, unfortunately so long unanswered, I reply Dr. Dalton always gave us our lessons in Chemistry in the room on the left on the ground floor.

The adjoining room on the same floor contained some apparatus, such as a pneumatic trough, but not much other, so far as I can remember. I never was in the upper room, except on occasion of his public lectures, when few attended. He was more proud of his English Grammar than of his New System of Chemical Philosophy. If you would like to have my copy of his Grammar I will send it to you.

For many years I kept the memoranda of his fees. One and sixpence a lesson ! !

When in acknowledgment of his skill and reputation and valuable advice in business my mother presented [him] with a cheque for £20, and I had the great pleasure of presenting it to him, he blushed and said, "but thou'l want some change out of this."

This at a time when an introduction in Paris would have been more valuable than Lord Palmerston's. I intended to present the memoranda of fees to the British Museum, but in changes of residence they have been lost.

Yours faithfully,
ADAM BEALEY.

Though so slight, Dr. Bealey's recollections of the great chemist show vividly the kind of man Dalton was. A chemist

of world-wide fame, content to charge eighteenpence a lesson, and having qualms of conscience about receiving a fee of £20!

Dr. Bealey was the son, I am informed by Mr. William Hewitson of Bury, of Adam Bealey (1781-1821), by his wife Mary Williams, who died at her son's house, in Tavistock Square, London, 15th February, 1858. His grandfather was Richard Bealey, of Radcliffe, and his great grandfather was Joseph Bealey, of Radcliffe. Adam Bealey, M.A., M.D., F.R.C.P., was born about 1813. He was educated at Cambridge, first at St. John's College, and afterwards at Queens'. He also studied at St. George's and Guy's Hospitals. For some time he was actively interested in a paper-works at Prestolee, belonging to the Bealey family. Later he was in practice as a physician, in Harrogate, and on retiring went to live at St. Leonard's-on-Sea, where he died 5th March, 1905, aged 92, leaving a widow and children.

Mr. H. S. HOLDEN, B.Sc., F.L.S., read a paper communicated by Prof. F. E. WEISS, D.Sc., F.L.S., entitled "**On an Abnormal Fertile Spike of** *Ophioglossum vulgatum.*"

Mr. R. L. TAYLOR, F.C.S., F.I.C., read a paper, written by Dr. A. N. MELDRUM, and communicated by Prof. H. B. DIXON, M.A., F.R.S., entitled "**The Development of the Atomic Theory: (4) Dalton's Physical Atomic Theory**"

Both papers are printed in full in the *Memoirs.*

Ordinary Meeting, January 24th, 1911.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the tables.

In accordance with a resolution passed by the Council earlier in the day the PRESIDENT made the following communication to the Members:—

"The Council has decided to make known to the Members of the Society the causes which have prevented the publication of the Wilde Lecture for 1910.

The lecture was delivered on March 22nd by Sir Thomas Henry Holland, who, on June 29th, received a letter from Messrs. Slater, Heelis & Co., Solicitors, acting for Dr. Henry Wilde, F.R.S., requesting the immediate delivery to the Society of the manuscript of the lecture for publication, or the return of the honorarium. This was followed on July 1st by an intimation from the same solicitors that at Dr. Wilde's request they had issued a writ against Sir Thomas Holland, who promptly instructed his Solicitors to accept service. This writ has not been served, but Sir Thomas Holland decided not to forward the manuscript unless the writ were withdrawn unconditionally.

Two other actions have been commenced by Dr. Wilde, one against the Society and the other against Dr. Hickling, one of the Secretaries. The latter, however, has been formally discontinued.

Under these circumstances the Council has reluctantly resolved that it is not desirable to nominate a Wilde Lecturer for 1911."

Mr. R. L. TAYLOR, F.C.S., F.I.C., read a paper, written by Dr. A. N. MELDRUM, and communicated by Prof. H. B. DIXON, M.A., F.R.S., entitled **"The Development of the Atomic Theory: (5) Dalton's Chemical Theory."**

Prof. J. E. PETAVEL, D.Sc., F.R.S., read a paper, written by Prof. A. H. GIBSON, D.Sc., entitled **"The Behaviour of Bodies Floating in a Free or a Forced Vortex."**

Both papers are printed in full in the *Memoirs*.

Ordinary Meeting, February 7th, 1911.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the tables.

Mr. J. H. WOLFENDEN, B.Sc., and Mr. H. E. SCHMITZ, M.A., B.Sc., were nominated auditors of the Society's accounts for the session 1910-1911.

Prof. W. BOYD DAWKINS, D.Sc., F.R.S., made a short communication on the origin of the Roman numerals I.-X., in which he suggested that they were derived from a system of numeration employed by the inhabitants of Crete during the Minoan civilisation. This conclusion was based on a comparison of the Roman numerals with a set of Minoan numerical symbols which Sir Arthur Evans, the distinguished excavator of Crete, had shown to Prof. Dawkins.

Mr. ROBERT COTTON, M.Sc., read a paper written by Prof. A. H. GIBSON, D.Sc., entitled "**The Manner of Motion of Water Flowing in a Curved Path.**"

Miss MARGARET C. MARCH, B.Sc., read a paper, communicated by Dr. George Hickling, entitled "**Studies in the Morphogenesis of certain Pelecypoda. II. The Ancestry of the Gibbosæ.**"

Both papers are printed in full in the *Memoirs*.

Ordinary Meeting, February 21st, 1911.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the tables.

Dr. ALFRED HOLT, M.A., read a paper entitled "**The Boric Acids.**"

The paper is printed in full in the *Memoirs*.

Mr. J. E. MYERS, B.Sc., read a paper, written in conjunction with Dr. A. HOLT, entitled "**The Hydration of Metaphosphoric Acid,**" of which the following is an abstract.

Experiments were described by which it was shown that pyrophosphoric acid is formed as an intermediate compound in the hydration of metaphosphoric acid. It was further shown that the hydration did not take place according to any simple scheme, and a method of estimating meta acid in a solution of all three varieties by means of barium chloride was described.

From the depression of the freezing point of aqueous solutions of various varieties of pyro and meta acids it appears that, when these acids are prepared by dehydration of orthophosphoric acid, there occurs association of the molecules, but when prepared by decomposition of the lead salts by hydrogen sulphide, simple molecules result. The peculiar "crackling" phenomenon which accompanies the solution of one form of meta acid was shown.

Ordinary Meeting, March 7th, 1911.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the tables.

The PRESIDENT referred to the loss the Society had sustained through the death of Professor J. H. van't Hoff, the celebrated Dutch Chemist, who had been an Honorary Member since 1892, and was present at the dinner in 1903 when the

Society celebrated the centenary of the discovery of Dalton's atomic theory. Professor van't Hoff was best known for his work on Stereo-isomerism, and the theory advanced by him to explain these phenomena is now generally accepted by chemists.

Professor G. ELLIOT SMITH, F.R.S., exhibited a cast of the Gibraltar skull—the most complete palæolithic skull known—now in the Museum of the Royal College of Surgeons. It was hewn out of a terrace of solid conglomerate limestone, under the north face of the Rock of Gibraltar, by the Royal Engineers in 1848; but was only cleaned of its matrix and properly displayed last year by Professor Arthur Keith, of the Royal College of Surgeons, to whom Prof. Elliot Smith was indebted for this cast. The outstanding features of this skull are (1) its small brain capacity (1060 cc.); (2) the great supra-orbital ridge of bone and the low receding forehead; (3) the great orbits and nose, larger than those of modern races; (4) the blown out appearance of the face due to the great development of the air spaces of the upper jaw; and (5) the very slight development of the mastoid process.

Professor E. RUTHERFORD, F.R.S., read a paper entitled "**The Scattering of the α and β Rays and the Structure of the Atom**," of which the following is an abstract.

It is well known that the α and β particles are deflected from their rectilinear path by encounters with the atoms of matter. On account of its smaller momentum and energy, the scattering of the β particles is in general far more pronounced than for the α particles. There seems to be no doubt that these swiftly moving particles actually pass through the atomic system, and a close study of the deflexions produced should throw light on the electrical structure of the atom. It has been usually assumed that the scattering observed is the result of a multitude of small scatterings. Sir J. J. Thomson (*Proc. Cam. Phil. Soc.*, 15, Pt. 5, 1910) has recently put forward a theory of small scattering, and the main conclusions of the theory have been experimentally examined by Crowther for β rays (*Proc. Roy. Soc.*,

84, p. 226, 1910). On this theory, the atom is supposed to consist of a positive sphere of electrification containing an equal quantity of negative electricity in the form of corpuscles. By comparison of theory with experiment, Crowther concluded that the number of corpuscles in an atom is equal to about three times its atomic weight in terms of hydrogen. There are, however, a number of experiments on scattering, which indicate that an α or β particle occasionally suffers a deflexion of more than 90° in a single encounter. For example, Geiger and Marsden (*Proc. Roy. Soc.*, 82, p. 495, 1909) found that a small fraction of the α particles incident on a thin foil of gold suffers a deflexion of more than a right angle. Such large deflexions cannot be explained on the theory of probability, taking into account the magnitude of the small scattering experimentally observed. It seems certain that these large deviations of the α particle are produced by a single atomic encounter.

In order to explain these and other results, it is necessary to assume that the electrified particle passes through an intense electric field within the atom. The scattering of the electrified particles is considered for a type of atom which consists of a central electric charge concentrated at the point and surrounded by a uniform spherical distribution of opposite electricity equal in amount. With this atomic arrangement, an α or β particle, when it passes close to the centre of the atom, suffers a large deflexion, although the probability of such large deflexions is small. On this theory, the fraction of the number of electrified particles which are deflected between an angle ϕ and $\phi + d\phi$ is given by $\frac{\pi}{4} ntb^2 \cot \phi/2 \operatorname{cosec}^2 \phi/2 d\phi$ where n is the number of atoms per unit volume of the scattering material, t the thickness of material supposed small, and $b = \frac{2NeE}{mu^2}$ where Ne is the charge at the centre of the atom, E the charge on the electrified particle, m its mass, and u its velocity.

It follows that the number of scattered particles per unit area for a constant distance from the point of incidence of the pencil of rays varies as $\operatorname{cosec}^4 \phi/2$. This law of distribution has

been experimentally tested by Geiger for α particles, and found to hold within the limit of experimental error.

From a consideration of general results on scattering by different materials, the central charge of the atom is found to be very nearly proportional to its atomic weight. The exact value of the central charge has not been determined, but for an atom of gold it corresponds to about 100 unit charges. From a comparison of the theories of large and small scattering, it is concluded that the effects are mainly controlled by the large scattering, especially when the fraction of the number of particles scattered through considerable angles is small. The results obtained by Crowther are for the most part explained by this theory of large scattering, although no doubt they are to a certain extent influenced by small scattering. It is concluded that for different materials the fraction of particles scattered through a large angle is proportional to NA^2 where N is the number of atoms per unit volume, and A the atomic weight of the material.

The main results of large scattering are independent of whether the central charge is positive or negative. It has not yet been found possible to settle this question of sign with certainty.

This theory has been found useful in explaining a number of results connected with the scattering and absorption of α and β particles by matter. The main deductions from the theory are at present under examination in the case of the α rays by Dr. Geiger using the scintillation method.

Dr. H. GEIGER read a paper entitled "**The Large Scattering of the α Particles,**" of which the following is an abstract.

Geiger and Marsden have shown that a small fraction of the α particles incident on a thin film of matter are so scattered that they emerge again on the side of incidence. In the present paper the fraction of the α particles scattered through various large angles by a thin gold foil has been experimentally

determined by the scintillation method. Radium emanation enclosed in a fine glass tube was used as a source. The microscope to which the zinc sulphide screen was attached moved round the arc of a circle; the distance between the scattering material and the screen was constant and equal to about 2 cms. The source of radiation, the scattering foil, and the screen were enclosed in a metal vessel which was exhausted to a low pressure. The number of α particles scattered through large angles up to 150° was first measured, and, as the emanation decayed, the number of small angles was successively determined. The number of scattered particles per unit area varied, when corrected for decay, nearly 300 times over the range of angles examined. The actual numbers of particles observed varied very approximately as $\mathrm{cosec}^4\phi/2$ where ϕ is the angle of deflection. This is the relation theoretically deduced by Professor Rutherford in the foregoing paper.

Experiments are in progress to examine the other deductions of the theory, and especially the variation of the amount of large scattering with thickness and nature of scattering foils and velocity of the α particles.

Mr. R. F. GWYTHER, M.A., read a paper entitled "**Can the Parts of a Heavy Body be supported by their Elastic Reactions only?**"

The paper is printed in full in the *Memoirs* under the title "**The Conditions that the Stresses in a Heavy Body should be purely Elastic Stresses.**"

Ordinary Meeting, March 21st, 1911.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the tables. The following were amongst the recent

donations to the Society's Library: "*Istar und Gilgamos. Babilonische Sage.*"—Aus dem Ungarischen des Árpád Zempléni ins Deutsche übertrag. von J. Lechner von der Lech (16mo., Budapest, 1911), presented by the author; "*Codex diplomaticus Lusatiæ superioris iii.*"...Hft. 6, herausg. von R. Jecht (8vo., Görlitz, 1910), presented by the Oberlausitzische Gesellschaft der Wissenschaften, Görlitz; "*Die Reise der deutschen Expedition zur Beobachtung des Venusdurchganges am 9 Dec. 1874*," von Dr. L. Weinek (4to., Prag, 1911), presented by the K. K. Sternwarte in Prag; "*Carl von Voit*," von O. Frank (4to., München, 1910), presented by the K. Akademie der Wissenschaften of Munich; "*Sixth Report on Research Work on...Typhoid bacilli*," by Dr. A. C. Houston (8vo., [London], 1910), presented by the Metropolitan Water Board; "*Rapport sur l'Expédition Polaire Néerlandaise...1882/83*," par M. Snellen et H. Ekama (fol., Utrecht, 1910), presented by the Kon. Nederland. Meteorolog. Institut, Utrecht; "*Les Prix Nobel, 1908*" (8vo., Stockholm, 1909), presented by the K. Svensk. Vetenskaps Akademie Stockholm; "*Report on a part of the N.W. Territories drained by the Winisk and Attawapiskat*," by W. McInnes (8vo., Ottawa, 1910), presented by the Geological Survey of Canada; "*Handbook of American Indians North of Mexico*," part 2, ed. by F. W. Hodge (8vo., Washington, 1910), presented by the Bureau of American Ethnology; and the following six pamphlets by Ch. Janet: "*Sur la Morphologie de l'Insecte*" (4to., Limoges, 1909), "*Sur l'Ontogénèse de l'Insecte*" (4to., Limoges, 1909), "*Note sur la Phylogénèse de l'Insecte*" (8vo., Rennes, 1909), "*Sur la Morphologie des Membranes Basales de l'Insecte*" (8vo., Beauvais, 1909), "*Sur la Parthénogénèse arrhénotoque de la Fourmi ouvrière*" (8vo., Beauvais, 1909), and "*Sur un Nématode qui se développe dans la tête de la Formica fusca*" (8vo., Beauvais, 1909), presented by the author.

Mr. WILLIAM THOMSON, F.R.S.E., F.C.S., read a paper entitled: **"On the Influence of Atmospheric Pressure and Humidity on Animal Metabolism."**

In a previous paper the author stated he had found that the percentage of Carbonic Acid gas contained in the exhaled air from the lungs was greater when breathing dry than when breathing damp air, also when breathing in Mountainous districts where the atmospheric pressure was low than when breathing in the Valley, and, again, was greater when breathing in the Valley than when breathing at the bottom of a deep Coal pit where the pressure is still greater. The experiments recorded in the present paper were made upon the exhaled air from three men and one boy, and upon guinea pigs and mice, and the results from all shew that, as a rule, when the barometer fell, the percentage of Carbonic Acid in the exhaled air rose, and, when the barometer rose, the percentage of Carbonic Acid fell. As the air became more moist the percentage of Carbonic Acid fell, and it rose when the air became drier.

There was a lower percentage of Carbonic Acid in the exhaled air when the weather was warm than when it was cold.

The paper will be published in full in the next volume.

Miss MARGARET C. MARCH, B.Sc., read a paper, communicated by Dr. HICKLING, entitled: "**Studies in the Morphogenesis of certain Pelecypoda. III. The Ornament of *Trigonia clavellata* and some of its Derivatives.**"

The paper is printed in full in the *Memoirs*.

General Meeting, April 4th, 1911.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

Mr. ARTHUR ADAMSON, A.R.C.S., Lecturer in Physics in the Municipal School of Technology, Manchester, and Mr. C. G. DARWIN, B.A., Reader in Mathematical Physics in the University of Manchester, were elected ordinary members of the Society.

Ordinary Meeting, April 4th, 1911.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the tables.

Professor F. E. WEISS, D.Sc., F.L.S., exhibited a hybrid of the Oxlip (*Primula elatior*) and the Primrose (*Primula acaulis*) collected by him in Cambridgeshire last year, where such plants are very common in the woods in which both the parental species occur. The hybrid bears its flowers in clusters on an erect scape as in the oxlip, but the flowers are much larger and paler, and resemble in their size and marking those of the primrose. The offspring of the hybrid (f_2 generation) showed a number of different forms, some resembling the parent hybrid but with a considerable range of variation in the size and colour of the flowers. Most of the plants bore all their flowers on scapes, but others only showed radical flowers and seemed therefore to have reverted to the primrose type. The scape must therefore be regarded as a dominant character, as it appears in the presumptive f_1 generation and also in the majority of plants of the f_2 generation. All the individuals of this generation have not flowered yet, but among the early flowering individuals were some possessing both radical and cauline flowers, and one of the primrose type with pure white petals.

Professor W. W. HALDANE GEE read a paper, written in conjunction with Mr. A. ADAMSON, A.R.C.S., entitled "**Dioptriemeters.**"

The paper is printed in full in the *Memoirs*.

Professor E. KNECHT, Ph.D., read a note "**On the Action of Hydrogen Peroxide on Quinone.**" It was shown that when hydrogen peroxide is allowed to act on quinone in presence of ammonia, the solution becomes heated and a

brisk evolution of oxygen takes place. On acidulating the solution and extracting with ether, hydroquinone was found to have been formed in considerable amount. Toluquinone behaves in a similar way to ordinary quinone.

Mr. R. L. TAYLOR, F.C.S., F.I.C., communicated a paper written by Dr. A. N. MELDRUM, entitled "**The Development of the Atomic Theory: (6) The Reception accorded to the Theory as advocated by Dalton.**"

The paper is printed in full in the *Memoirs*.

The reading of Dr. MELDRUM's paper on "**The Development of the Atomic Theory: (7) The Rival Claims of William Higgins and John Dalton to the Chemical Theory,**" was postponed till the Meeting of April 25th.

Annual General Meeting, April 25th, 1911.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

The Annual Report of the Council and the Statement of Accounts were presented, and it was resolved:—"That the Annual Report, together with the Statement of Accounts, be adopted, and that they be printed in the Society's *Proceedings*.

Mr. T. G. B. OSBORN and Mr. E. L. RHEAD were appointed Scrutineers of the balloting papers.

The following members were elected officers of the Society and members of the Council for the ensuing year:—

President: F. E. WEISS, D.Sc., F.L.S.

Vice-Presidents: FRANCIS JONES, M.Sc., F.R.S.E.; ERNEST RUTHERFORD, D.Sc., F.R.S.; ARTHUR SCHUSTER, Sc.D., Ph.D., F.R.S.; FRANCIS NICHOLSON, F.Z.S.

Secretaries: R. L. TAYLOR, F.C.S., F.I.C.; GEORGE HICKLING, D.Sc.

Treasurer: WILLIAM HENRY TODD.

Librarian: C. L. BARNES, M.A.

Other Members of the Council: EDMUND KNECHT, Ph.D.; WILLIAM BURTON, M.A., F.C.S.; SYDNEY J. HICKSON, M.A., D.Sc., F.R.S.; Sir THOMAS H. HOLLAND, K.C.I.E., D.Sc., F.R.S.; THOMAS THORP, F.R.A.S.; and T. A. COWARD, F.Z.S.

Ordinary Meeting, April 25th, 1911.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the tables. The following were amongst the recent donations to the Society's Library: "*Synopsis of the Sections Microporus, Tabacinus and Funales of the Genus Polystictus*," and "*Synopsis of the Genus Hexagona*," by C. G. Lloyd (8vo., Cincinnati, 1910), presented by the Lloyd Library; "*Catalogue of Egyptian Antiquities of the XII. and XVIII. Dynasties in the Manchester Museum*," by A. S. Griffith (8vo., Manchester, 1910), and "*Outline Classification of the Animal Kingdom*," 4th ed., by S. J. Hickson (8vo., Manchester, 1911), presented by the Museum; "*A Herbert Bibliography*," by G. H. Palmer (8vo., Cambridge, Mass., 1911), presented by Harvard University Library; "*Photography*," by A. Brothers, 2nd ed. (8vo., London, 1899), presented by the author; "*Nejstarší Breviář Chrvatsko-Hlaholský*," by J. Vajs (8vo., Praze, 1910), and "*Untersuchungen über den Lichtwechsel älterer veränderlichen Sterne*," vol. 1, von Vojtěch Šafařík (fol., Prag, 1910), presented by the K. Böhmische Gesellschaft, Prague, and "*Life and Scientific Work of Peter*

Guthrie Tait," by C. G. Knott (4to., Cambridge, 1911), presented by Mr. W. A. Tait.

Prof. F. E. Weiss, D.Sc., F.L.S., exhibited a specimen of the fungus *Gymnosporangium* parasitic on the common Juniper. This fungus appears in spring in the form of orange-coloured, finger-like processes on the stem and branches of the Juniper, and its spores are then carried by the wind to the leaves of the Mountain Ash or Hawthorn, on which it lives parasitically during the summer. It completes its life-history by re-infecting the Juniper in the autumn.

Dr. G. Hickling read a paper, written by Dr. Henry Wilde, F.R.S., entitled "**On the Periodic Times of Saturn's Rings.**"

The paper is printed in full in the *Memoirs*.

Mr. R. L. Taylor, F.C.S., F.I.C., communicated a paper, written by Dr. A. N. Meldrum, entitled "**The Development of the Atomic Theory: (7) The Rival Claims of William Higgins and John Dalton to the Chemical Theory.**"

The paper is printed in the *Memoirs*.

General Meeting, May 9th, 1911.

Mr. Francis Jones, M.Sc., F.R.S.E, Vice-President, in the Chair.

Mr. Henry Gwyn Jeffreys Moseley, Lecturer in Physics in the Victoria University of Manchester, *Dunwood House, Withington*, and Mr. Gilbert Cook, Vulcan Research Fellow in Engineering in the Victoria University of Manchester, *8, Clarendon Road, Garston, Liverpool*, were elected ordinary members of the Society.

Ordinary Meeting, May 9th, 1911.

Professor F. E. WEISS, D.Sc., F.L.S., President,
in the Chair.

The thanks of the members were voted to the donors of the books upon the tables.

Mr. ERNEST F. LANGE, M.I.Mech.E., F.C.S., read a paper entitled "**Some Remarkable Steel Crystals.**" The paper is printed in the *Memoirs* under the title "**An Account of some Remarkable Steel Crystals, along with some Notes on the Crystalline Structure of Steel.**"

Professor S. J. HICKSON, D.Sc., F.R.S., read a paper entitled "**On a specimen of** *Osteocella Septentrionalis* (Gray)."

The papers are printed in full in the *Memoirs.*

Annual Report of the Council, April, 1911.

The Society began the session with an ordinary membership of 145. During the present session nineteen new members have joined the Society. Thirteen resignations have been received. This will leave on the roll at the end of the session 151 ordinary members. One honorary member also has been elected, *viz.:* Geh. Professor Dr. WALTER NERNST. The Society has lost, by death, three honorary members, *viz.:* Professor STANISLAO CANNIZZARO, For.Mem.R.S., Professor J. H. VAN'T HOFF, Ph.D., For.Mem.R.S., and Sir WILLIAM HUGGINS, O.M., K.C.B., F.R.S., and one corresponding member, *viz.*, the Rev. ROBERT HARLEY, F.R.S. Memorial notices of these gentlemen appear at the end of this report.

The average attendance at the meetings was 21, the same as for the session 1909-10.

The Society commenced the session with a balance in hand of £368. 3s. 4d., from all sources, this amount being made up of the following balances:—

At the credit of	General Fund	£102	9	9
„ „	Wilde Endowment Fund...	179	9	7
„ „	Joule Memorial Fund.....	86	4	0
		£368	3	4

The total balance in hand at the close of the session amounted to £304. 7s. 8d., and the amounts standing at the

credit of the separate accounts, on the 31st March, 1911, are the following:—

At the credit of General Fund	£62	11	6
„ „ Wilde Endowment Fund...	149	12	4
„ „ Joule Memorial Fund	92	3	10
Balance 31st March, 1911.....	£304	7	8

The Wilde Endowment Fund, which is kept as a separate banking account, shows a balance of £149. 12s. 4d. in its favour, as against £179. 9s. 7d. at the beginning of the financial year, the receipts from the invested funds being the same as those for the previous year.

The Librarian reports that during the session 704 volumes have been stamped, catalogued and pressmarked, 656 of these being serials, and 48 separate works. There have been written 189 catalogue cards, 125 for serials, and 64 for separate works. The total number of volumes catalogued to date is 33,082 for which 11,731 cards have been written.

Satisfactory use is made of the library for reference purposes. During the session, 185 volumes have been borrowed from the library, as compared with 202 in the previous session.

Further attention has been given to the completion of sets, 46 volumes and parts having been obtained, which complete two sets and partly complete two others. These were presented by the societies publishing them.

A larger amount of binding has been done this session, 214 volumes having been bound in 167.

A record of the accessions to the library shows that, from April, 1910, to March, 1911, 758 serials and 67 separate works were received, a total of 825 volumes. The donations during

the session (exclusive of the usual exchanges) amount to 66 volumes and 152 dissertations; one volume has been purchased (in addition to the periodicals on the regular subscription list).

The following new serial publications have been received during the past session :—*University of Missouri Studies. Literary and Linguistic Series; Science Conspectus*, published by the Massachusetts Institute of Technology; and *Mathematical Notes*, published by the Edinburgh Mathematical Society.

The Library has also been presented by the U.S. Geological Survey with a copy of one hundred and fifty-eight folios of the 'Geologic Atlas of the United States.'

A new and complete catalogue of the serial publications received by the Society has been in progress for several months, and will shortly be issued. Some idea of its extent may be gathered from the fact that the total number of publications actually in progress is 808, of which 186 are from the British Islands, 165 from the U.S.A., 95 from Germany, 60 from France, 40 from Italy, 34 from Austria-Hungary, and so on. The problem of finding shelf room for the enormous mass of literature possessed by the Society will ere long become urgent.

The Society is indebted to Mr. Francis Nicholson, F.Z.S., for presenting to it three letters written to him in 1894 and 1902 by Dr. Adam Bealey, a former pupil of Dalton, explaining the circumstances under which Dr. Bealey gave to the Society a small bust of Dalton now exhibited in one of its rooms. The letters also give interesting glimpses of Dalton himself, and show what small fees the great scientist charged for the lessons he gave his pupils.

The publication of the *Memoirs and Proceedings* has been continued under the supervision of the Editorial Committee.

During the summer extensive decorations and repairs were carried out on the Society's premises, and the Society is greatly

indebted to Dr. H. Wilde, F.R.S., for the time and attention he bestowed in undertaking the business arrangements and supervision of them. The cost, amounting to £110. 13s. 5d., has been charged to the Wilde Endowment Fund.

It is with great regret that the Council report that Mr. Arthur McDougall has intimated his intention of resigning the Treasurership at the end of the present session, and desires to record its thanks for his care of the Society's finances during the eight years that he has held office. Mr. McDougall has been reluctantly compelled to take this step by the urgency of his health, the state of which has for some time made him wish to be relieved of the duties of his office, and he latterly only continued to discharge these at the earnest request of his fellow members of Council and at some sacrifice to himself.

Stanislao Cannizzaro, an honorary member of the Society from the year 1888, was born in 1826, and was a native of Palermo, in Sicily. After having studied medicine in his native town for four years, he turned his attention to chemistry, and worked at Pisa as assistant to Piria. On the outbreak of the Sicilian revolution, in 1848, he acted as an officer of artillery, and was elected a Deputy to the Sicilian Parliament. When the revolution was crushed in the next year, he escaped to Marseilles, and made his way to Paris.

In Paris he worked in Chevreul's laboratory, making an investigation along with Cloëz of the substance cyananide. In the year 1851 he was appointed Professor of Chemistry at Alexandria; from there he went to Genoa in 1855, and to Palermo in 1861. In 1871 he was elected to the Chair of Chemistry at Rome, and this position he held to the end of his life. He served his country also as a member of the Italian Senate, of which he became Vice-President, and as a member of the Council of Public Instruction. He died in the year 1910.

The experimental work which he published was chiefly in the region of organic chemistry. An important reaction which he discovered, is that in which an aromatic aldehyde is converted by treatment with caustic potash into the corresponding acid and alcohol: benzaldehyde, for instance, yields benzoic acid and benzylic alcohol. One may mention also the series of researches which he carried out on santonin and its derivatives.

Cannizzaro was greatest as a teacher of chemistry. At the time he began his career the science was in a chaotic state. The objection to the atomic theory, as that theory was understood between the years 1808 and 1860, was that the chemical formulæ of substances and the atomic weights of the elements were decided in an arbitrary way. Thus there had arisen three great systems of chemical formulæ, each with much in its favour, that of Berzelius, that of Gmelin, and that of Gerhardt and Laurent. The attempts which were made at compromise between these different systems only resulted in heightening confusion.

Cannizzaro's contribution to the philosophy of chemistry, which must make his fame enduring, was that he showed how to avoid all this confusion. He described his system of chemistry, though not till he had tested it amongst his own students, under the title, "Sketch of a Course of Chemical Philosophy."* His method was to use the hypothesis of his countryman, Avogadro, as a means of arriving at the molecular weights of substances, whether elementary or compound, and to make the molecular weights thus obtained the basis for determining the atomic weights of the elements. The other atomic weight methods, specific heat, isomorphism, and chemical analogy were, as he showed, simply auxiliary methods.

In 1860, two years after the publication of these ideas, a conference of chemists met at Carlsruhe for the express purpose of considering the state of confusion into which chemistry had

* *Il Nuovo Cimento*, vol. 7, pp. 321-366, 1858. Translated in No. 18 of the *Alembic Club Reprints*.

fallen. It was attended by some one hundred and forty men of science, including Liebig, Wöhler, Kekulé, Kopp, Bunsen, Odling, Roscoe, Dumas, Wurtz, and Cannizzaro.

The pessimism of the opening speakers gave Cannizzaro his opportunity. When Dumas declared that organic and inorganic chemistry were two distinct sciences, Cannizzaro was able to maintain the unity of the science, for he showed that both inorganic and organic chemistry ought to be submitted to Avogadro's hypothesis as a controlling principle. Kekulé made the declaration that the physical molecule and the chemical were not always identical, and that purely chemical researches could be carried on independently of physical considerations. In reply, Cannizzaro expressed his opinion that the physical and chemical molecules were absolutely identical, and he showed that the best way of establishing the molecular weight of a substance was by means of its vapour density. The hour and the man had come. Cannizzaro's two speeches constituted him the leader of the Conference. His suggestions, immensely aided by the dramatic circumstances in which they were made, were adopted by many of his hearers. His system of chemical formulæ and atomic weights, the one still in use, rapidly supplanted the old systems. And not only was the change good in itself, by reason of the clearness which it introduced into chemistry, but it led to unexpected advantages. It led to the establishment of three great doctrines. In the first place, the doctrine of gaseous dissociation was a natural outcome of Cannizzaro's teaching. In the next place, with regard to the doctrine of Valency, Frankland declared that till the atomic weights were placed on "their present consistent basis, the satisfactory development of the doctrine was impossible." Lastly, the periodic classification of the elements, in the judgment of Newlands and of Mendeléeff, could not have been worked out under any other system than that of Cannizzaro.

One is glad to think that the priceless service which Cannizzaro thus rendered to chemistry was widely recognised.

In this country the honours paid to him included the award of the Copley Medal by the Royal Society in 1891, and the invitation to deliver the "Faraday Lecture" to the Chemical Society of London. This invitation he accepted, and the lecture which he gave, under the modest title "Some Points on the Theoretic Teaching of Chemistry,"* is a lasting proof of what a great and winning teacher of chemical philosophy Italy produced in him.

A. N. M.

The Rev. Robert Harley, Hon. M.A. (Oxon.), F.R.S., &c., &c., who died at Westbourne Road, Forest Hill, London, on July 27th, 1910, had been a corresponding member of the Society since April 30th, 1850.

Mr. Harley was born in Liverpool, January 23rd, 1828, and was the son of a Wesleyan minister. His interest in and capacity for mathematics developed so suddenly that, though he had passed his fourteenth year before mastering the multiplication table, he was at sixteen mathematical master in a school at Seacombe. After being a teacher for several years, Mr. Harley became a divinity student at Airedale College, Bradford (now the United College, Bradford), and in 1854, minister of the Congregational Church at Brighouse, a position he retained for fourteen years, during the last four of which he was also tutor in mathematics and logic at Airedale College. In 1868 he accepted a call to Bond Street Chapel, Leicester. During the four years he held this pastorate, he took an active part in the public work of the town, serving on the School Board, and being honorary curator of the town museum. From 1872 to 1881 he was vice-principal and chaplain of Mill Hill School. He was principal of Huddersfield College from 1882 to 1885, and in 1886 he removed to Oxford, where he was minister of George Street Congregational Church. He received the honorary degree of M.A. in 1886, and took the leading part in the foundation of the

* *Trans. Chem. Soc.*, vol. 25, p. 941, 1872.

Oxford Mathematical Society. Ill health compelled him to take a rest in 1890, and he visited Australia, taking temporary charge of a church in Sydney. After his return he became, in 1892, minister of Heath, near Halifax. In 1895 he retired from the ministry, and afterwards resided at Forest Hill, eagerly pursuing to the last his mathematical studies, and doing much honorary preaching, temperance, and philanthropic work.

It was as a mathematician that Mr. Harley attained distinction, but science had never more than a secondary claim on his time. Of his mathematical work *Nature* says:—"The application of mathematics to logic, as developed by George Boole, captivated his intelligence, and he became the most notable of Boole's admirers and followers, as also his biographer. His greatest mathematical achievements were, however, in another field. The unsolved problem of the solution of quintic equations fascinated him. Having once granted the impossibility of the solution by radicals, he proceeded to exhibit with remarkable power and patience the place of certain sextic resolvents in connection with such equations. Simultaneously, the late Sir James Cockle was engaged on like work; but Harley was the clearer writer on the difficult subject. Their work, and in particular Harley's, was welcomed enthusiastically by Cayley, who himself took it up and continued it. All three probably were not aware at the time that certain continental writers had possessed some of their ideas beforehand; but everyone must recognise that Harley's development of the ideas was masterly. It secured for him the Fellowship of the Royal Society in 1863."

Mr. Harley was twice secretary, and three times vice-president, of the "A" Section of the British Association.

His contributions to the literature of pure mathematics and symbolic logic were numerous. Of these, the following were contributed to this Society:—

Papers:—

"On Impossible and certain other Surd Equations." (1851.) *Mem.* (2) ix. 207.

"On the Method of Symmetric Products, and its Application to the Finite Algebraic Solution of Equations." (1859.) *Mem.* (2) xv. 172.

"On a certain Class of Linear Differential Equations." (1862.) *Mem.* (3) ii. 232.

"On Bring's Reduction of the Equation of the Fifth Degree to a Trinomial Form." (1863.) *Proc.* iii. 69.

"On Recent Researches on the Theory of Equations." (1863.) *Proc.* iii. 173.

"On the Rev. T. P. Kirkman's Method of Resolving Algebraic Equations." (1868). *Proc.* viii. 4.

"On the Interchange of two Differential Resolvents." (1891.) *Mem.* (4) v. 79.

Minor Communications:—

"On the Theory of the Transcendental Solution of Algebraic Equations." (1862.) *Proc.* ii. 181, 199 and 237.

"On Linear Differential Equations." (1862.) *Proc.* iii. 17.

"Remarks on Mr. J. J. Murphy's Paper 'On the Quantification of the Predicate, and on the Interpretation of Boole's Logical Symbols.'" (1883.) *Proc.* xxiii. 36.

"Obituary Notice of Sir James Cockle." (1895.) *Mem.* (4) ix. 215.

F. N.

By the death of Professor J. H. VAN'T HOFF, which took place at Steglitz, near Berlin, on March 1st, the world of science has lost one of its most brilliant investigators and careful and inspiring teachers. All who have had the privilege of working under the direction of Professor van't Hoff will bear testimony to the wonderful inspiration given by association with him, whilst his course of lectures on physical chemistry, given at the University of Berlin, will always be remembered, not only for the breadth of treatment, but also for the simple and clear manner in which they were delivered.

Jacobus Henricus van't Hoff was born in Rotterdam, on August 30th, 1850. At the age of nineteen he entered the Polytechnicum at Delft, and after passing through the technological course, proceeded to the University of Leyden, continuing his studies later in Bonn, under Kekulé, and in Paris, under Wurtz. After a very short career as docent, van't Hoff was appointed professor of chemistry at the University of

Amsterdam, a post he held for eighteen years. In 1896 he was elected a professor in the University of Berlin, delivering lectures on physical chemistry in the University, but carrying out his research work with his pupils in a private laboratory in Charlottenburg.

In a brief account it is impossible to do justice to the impetus given to research, especially in the domain of physical chemistry, by the large number of investigations carried out by him. Three subjects, however, stand out most prominently. The earlier work of van't Hoff was chiefly in the field of organic chemistry, and in this connection his genius soon led to the formulation of a new idea with regard to structural organic chemistry. In 1874 a short pamphlet was published, which put forward the idea of three dimensional space formulæ for organic compounds, and also expressed the now well known relation between optical activity and the presence of an asymmetric carbon atom. In the following year the book "La Chemie dans l'espace" appeared. This book contained a clear and full account of the "tetrahedral" carbon atom, and may be said to have laid the foundations of the important science of stereochemistry.

The work, however, which will be always pre-eminently associated with the name of van't Hoff appeared in 1886. In this year van't Hoff put forward his views on the analogy between the laws relating to dilute solutions and the well known laws relating to gases. Using a number of experimental results obtained by Pfeffer, Traube, and others, in their investigations on osmotic pressure, he was able to show the applicability of the gas laws to dilute solution, and further that this close relation in the behaviour of dilute solutions and gases was thermodynamically necessary. It may be said that this development, along with the electrolytic dissociation theory of Arrhenius, gave the first satisfactory theory of the general properties and relations of dilute solutions.

The third great development due to van't Hoff was carried out in Berlin. A very careful and complete investigation into the conditions under which various simple and double salts crystallize, led to a clear statement of the general conditions governing this class of heterogeneous equilibria. With this work as a basis, he commenced the very laborious investigation into the conditions of formation of the Stassfurt salt deposits. A collected account of these researches, which were carried out in conjunction with the late Dr. Meyerhoffer and a number of pupils, has been published under the title of "Zur Bildung der ozeanischen Salzablagerungen."

Even in such a brief account as is here presented, mention should be made of the publication, in 1884, of the celebrated "Etudes de Dynamique chimique," in which the author gives an account of his researches on the quantitative relations met with in the course of chemical reactions, and especially on the conditions of equilibrium.

Many will undoubtedly remember his visit to Manchester on the occasion of the celebration of the centenary of Dalton's atomic theory. During his visit the honorary degree of Doctor of Science was conferred upon him by the University of Manchester, thus adding one more to the long list of honorary degrees of which he has been the recipient. Many other honours, including the award of the Nobel Prize in 1901, were bestowed upon him by nearly all the well known scientific societies, but undoubtedly he will be remembered by chemists in general as a great pioneer and investigator, and by his pupils not only as a brilliant chemist, but also as a beloved and successful teacher.

N. S.

By the death of Sir William Huggins, O.M., K.C.B., F.R.S., on May 12th, 1910, this Society lost a distinguished honorary member, who for many years was one of the foremost

pioneers in the new branch of science now known as Astro-Physics.

Born on February 7th, 1824, he received his early education at the City of London School, and continued his training in mathematics, classics, and modern languages with the help of private masters. He also studied at home various branches of science, purchasing or constructing for himself the apparatus required for his experiments.

Finally he decided to devote himself to astronomy, and in 1855 he erected the observatory attached to his residence at Upper Tulse Hill, in London, where his life's work was carried out. He began in the usual way by measuring positions of stars and he also made drawings of the planets. Work of this kind, however, did not satisfy him; his mind was seeking for new methods of research. He describes with what pleasure he heard about this time of Kirchhoff's discovery of the meaning of the Fraunhofer lines in the solar spectrum. This discovery came to him as an inspiration, opening up a new line of investigation, which he determined to extend, if possible, to all parts of the visible universe.

The task of extending Kirchhoff's work to the spectra of the stars appeared very formidable, owing to the faintness of their light; but the stars have one great advantage over the sun, that the brightness of a star image increases with the light collecting power of the telescope, and Huggins found it was possible, with a spectroscope attached to the eyepiece end of his eight-inch refractor, to make detailed comparisons of the dark lines in a number of stellar spectra with the spectrum lines of chemical elements.

His standard of scientific work was very high. Finding the existing charts of spectra inconvenient for his purpose, he devoted the greater part of 1863 to mapping, with a train of six prisms, the spark spectra of twenty-six elements, using as a reference scale the spark spectrum lines of air. Being well

equipped by his preliminary training to enter the new field of investigation, and employing such sound methods, he was soon rewarded by a rich harvest of results, and for a number of years he was able to surprise the scientific world with important discoveries.

One of the most striking of these was his discovery of the simple bright line spectrum of nebulæ, which he observed with feelings of awe, for the first time, on the evening of August the 29th, 1864. At that time there was a growing belief that all nebulæ would ultimately be resolved into innumerable stars. He was able, however, to distinguish at once between nebulæ, giving a bright line spectrum, and star clusters, giving a continuous spectrum crossed by dark lines.

When the difficulties of one line of investigation had been overcome, Huggins was always ready to leave the smooth path for fresh difficulties. Almost from the beginning of his stellar work he had looked forward to so far perfecting his instruments and methods as to be able to detect displacements of the spectrum lines of the stars due to their motion towards or away from the earth. Finally, in 1868, he was able to announce that he had measured the velocity of Sirius. It is not surprising that many astronomers, who at that time had not adopted spectroscopic methods, did not regard his early work in this direction seriously.

Huggins was quick to appreciate the advantages offered by photography. His first attempts to photograph spectra were indeed made before the dry plate was invented, and as early as 1876 he was employing an Iceland spar and quartz spectroscope attached to a reflecting telescope to extend his photographs into the ultra-violet region.

By his marriage in 1875 he secured an enthusiastic assistant in his scientific work. Lady Huggins collaborated with her husband in much of his later work, and the "Atlas of Representative Spectra," published in 1899, is a beautiful monument, both in a scientific and artistic sense, of their joint labour.

To the end of his life Huggins remained remarkably accessible to new ideas, and when radium was discovered he undertook the task of photographing the spectrum of its spontaneous luminosity.

He became President of the Royal Society, and indeed received almost every scientific honour that could be conferred upon him.

In 1897 he was made a Knight Commander of the Bath, and on the foundation of the Order of Merit, at the beginning of King Edward's reign, he was one of the first to be enrolled.

He was elected an Honorary Member of this Society in 1869.

H. S.

NOTE.—The Treasurer's Accounts of the Session 1910-1911, of which the following pages are summaries, have been endorsed as follows:

April 11th, 1911. Audited and found correct.

We have also seen, at this date, the certificates of the following Stocks held in the name of the Society:—£1,225 Great Western Railway Company 5% Consolidated Preference Stock, Nos. 12,293, 12,294, and 12,323; £258 Twenty years' loan to the Manchester Corporation, redeemable 25th March, 1914 (No. 1,564); £7,500 Gas Light and Coke Company Ordinary Stock (No. 6,389); and the deeds of the Natural History Fund, of the Wilde Endowment Fund, those conveying the land on which the Society's premises stand, and the Declaration of Trust.

Leases and Conveyance dated as follow:—

22nd Sept., 1797.
23rd Sept., 1797.
25th Dec., 1799.
" " "
22nd Dec., 1820.
23rd Dec., 1820.

Declarations of Trust:—

24th June, 1801.
23rd Dec., 1820.
30th April, 1851.
8th Jan., 1878.

We have also verified the balances of the various accounts with the bankers' pass books.

(Signed) J. H. WOLFENDEN.
H. E. SCHMITZ.

MANCHESTER LITERARY AI

Dr. *Arthur McDougall, Treasurer, in Account with*

	£ s. d.	£ s
To Cash in hand, 1st April, 1910		150 1
To Members' Subscriptions :—		
Half Subscriptions, 1910-11, 14 at £1. 1s. od.	14 14 0	
Subscriptions :— 1908-09, 3 ,, £2. 2s. od.	6 6 0	
,, 1909-10, 7 ,, ,, ...	14 14 0	
,, 1910-11, 116 ,, ,, ...	243 12 0	
,, 1911-12, 1 ,, ,, ...	2 2 0	
		231 8
To Transfers from the Wilde Endowment Fund	.. ..	80 9
To Transfer from Dalton Tomb Fund	.. .	40 1
To Sale of Publications	.. ..	14 9
To Dividends :—		
Natural History Fund	57 13 6	
Joule Memorial Fund	7 5 10	
		64 19
To Income Tax Refunded :—		
Natural History Fund	3 11 6	
Joule Memorial Fund	0 9 0	
Wilde Endowment Fund	20 8 4	
		24 8
To H. Sidebottom for plates	.. ..	3 0
		£658 17

NATURAL HISTOI

	£ s.
To Balance, 1st April, 1910	15 3
To Dividends on £1,225 Great Western Railway Company's Stock	57 13
To Remission of Income Tax, 1910..	3 11
	£76 8

JOULE MEMORIA

	£ s.
To Balance, 1st April, 1910	86 4
To Dividends on £258 Loan to Manchester Corporation	7 5
To Remission of Income Tax, 1910	0 9
	£93 18

WILDE ENDOWMEN

	£ s.
To Balance 1st April, 1910	179 9
To Dividends on £7,500 Gas Light and Coke Company's Ordinary Stock	329 11
To Remission of Income Tax, 1910	20 8
To Bank Interest	2 10
	£531 19

[ILOSOPHICAL SOCIETY.

iety, from 1st April, 1910, to 31st March, 1911. Cr.

	£ s. d.	£ s. d.
Charges on Property :—		
Chief Rent (Income Tax deducted)	12 3 4	
Income Tax	1 10 0	
Insurance against Fire	11 0 0	
House Expenditure :—		24 13 4
Coals, Gas, Electric Light, Water, &c.	38 10 2	
Tea, Coffee, &c., at Meetings	17 1 9	
Cleaning, Sweeping Chimneys, &c.	5 9 0½	
Cleaning and Repairing Pictures	5 0 0	
Replacements of mantles, crockery, dusters, etc.	2 1 4	
Administrative Charges :—		68 2 3½
Housekeeper	65 0 0	
Postages, and Carriage of Parcels and of "Memoirs"	29 11 5	
Stationery, Cheques, Receipts, and Engrossing	6 9 8	
Printing Circulars, Reports, &c.	15 14 0	
Extra attendance at Meetings, and during housekeeper's holidays	3 15 0	
Insurance against Liability	0 12 0	
Gratuities to Mrs. Kelly	2 13 0	
Miscellaneous Expenses	2 16 9½	
		126 11 10½
Publishing :—		
Printing "Memoirs and Proceedings"	137 2 8	
Illustrations for "Memoirs" (except Nat. Hist. papers)	3 19 1	
Library :—		141 1 9
Books and Periodicals (except those charged to Natural History Fund) ..	46 9 6	
Periodicals formerly subscribed for by the Microscopical and Natural History Section	4 9 6	
Catalogue Cards and Cabinet	3 4 10	
Natural History Fund :—		54 3 10
(Items shown in the Balance Sheet of this Fund below)	.. ..	65 3 9
Joule Memorial Fund :—		
(Items shown in the Balance Sheet of this Fund below)	.. ..	1 15 0
Wilde Endowment Fund (Income Tax refunded)	.. ..	20 8 4
Subscription refunded	.. ..	2 2 0
Balance at Williams Deacon's Bank, 1st April, 1911	144 15 4	
,, in Treasurer's hands	10 0 0	
		154 15 4
		£658 17 6

JND, 1910—1911. (Included in the General Account, above.)

	£ s. d.
Natural History Books and Periodicals	48 5 8
Illustrations for papers on Nat. Hist. in "Memoirs"	14 9 0
Binding Books	2 9 1
Balance, 1st April, 1911	11 4 8
	£76 8 5

JND, 1910—1911. (Included in the General Account, above.)

	£ s. d.
Repairing Chronometer belonging to Dr. J. P. Joule	1 15 0
Balance, 1st April, 1911	92 3 10
	£93 18 10

JND, 1910—1911.

	£ s. d.
Assistant Secretary's Salary, April, 1910, to March, 1911	150 0 0
Maintenance of Society's Library :—	
Binding and Repairing Books	23 9 2
Repairs and Improvements to Society's Premises	113 0 0
Cleaning Carpets and Curtains	12 9 11
Upholstering Chairs	2 17 0
Transfers to Society's Funds	80 9 0
Cheque Book	0 2 6
Balance at District Bank, 1st April, 1911	149 12 4
	£531 19 11

THE COUNCIL

AND MEMBERS

OF THE

MANCHESTER

LITERARY AND PHILOSOPHICAL SOCIETY.

(*Corrected to September 8th, 1911.*)

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Prof. F. E. WEISS, D.Sc., F.L.S.

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FRANCIS JONES, M.Sc., F.R.S.E., F.C.S.
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THOMAS THORP, F.R.A.S.
T. A. COWARD, F.Z.S.

Assistant Secretary and Librarian.

A. P. HUNT, B.A.

ORDINARY MEMBERS.

Date of Election.

1911, April 4. Adamson, Arthur, A.R.C.S., Lecturer in Physics in the Municipal School of Technology, Manchester. *Technical School, Sackville Street, Manchester.*

1901, Dec. 10. Adamson, Harold. *Oaklands Cottage, Godley, near Manchester.*

1902, Mar. 18. Allen, J. Fenwick. 147, *Withington Road, Whalley Range, Manchester.*

1870, Dec. 13. Angell, John, F.C.S., F.I.C. 6, *Beaconsfield, Derby Road, Withington, Manchester.*

1865, Nov. 14. Bailey, Charles, M.Sc., F.L.S. *Haymesgarth, Cleeve Hill S.O., Gloucestershire.*

1888, Feb. 7. Bailey, Alderman Sir William H., M.I.Mech.E. *Sale Hall, Sale, Cheshire.*

1895, Jan. 8. Barnes, Charles L., M.A. 151, *Plymouth Grove, Chorlton-on-Medlock, Manchester.*

1903, Oct. 20. Barnes, Jonathan, F.G.S. *South Cliff House*, 301, *Great Clowes Street, Higher Broughton, Manchester.*

1910, Oct. 18. Beattie, Robert, D.Sc., Lecturer in Electrotechnics in the University of Manchester. *The University, Manchester.*

1895, Mar. 5. Behrens, Gustav. *Holly Royde, Withington, Manchester.*

1898, Nov. 29. Behrens, Walter L. 22, *Oxford Street, Manchester.*

1868, Dec. 15. Bickham, Spencer H., F.L.S. *Underdown, Ledbury.*

1875, Nov. 16. Boyd, John. *Barton House*, 11, *Didsbury Park, Didsbury, Manchester.*

1889, Oct. 15. Bradley, Nathaniel, F.C.S. *Sunnyside, Whalley Range, Manchester.*

1861, April 2. Brogden, Henry, F.G.S., M.I.Mech.E. *Hale Lodge, Altrincham, Cheshire.*

1889, April 16. Brooks, Samuel Herbert. *Slade House, Levenshulme, Manchester.*

1910, Nov. 1. Broome, James S., Science Teacher in the Salford Secondary School. 18, *Seedley Park Road, Pendleton, Manchester.*

1860, Jan. 24. Brothers, Alfred. *Handforth, near Manchester.*

1886, April 6. Brown, Alfred, M.A., M.D. *Sandycroft, Higher Broughton, Manchester.*

1889, Jan. 8. Brownell, Thomas William, F.R.A.S. 64, *Upper Brook Street, Manchester.*

Date of Election.

1889, Oct. 15. Budenberg, C. F., M.Sc., M.I.Mech.E. *Bowdon Lane, Marple, Cheshire.*

1911, Jan. 10. Burt, Frank Playfair, D.Sc.(Lond.), Assistant Lecturer and Demonstrator in Chemistry in the Victoria University of Manchester. 5, *Beaconsfield, Derby Road, Withington, Manchester.*

1906, Feb. 27. Burton, Joseph, A.R.C.S. Dublin. *Tile Works, Clifton Junction, near Manchester.*

1894, Nov. 13. Burton, William, M.A., F.C.S. *The Hollies, Clifton Junction, near Manchester.*

1904, Oct. 18. Campion, George Goring, L.D.S. 264, *Oxford Street, Manchester.*

1907, Jan. 15. Carpenter, H. C. H., M.A., Ph.D., Professor of Metallurgy in the University of Manchester. 11, *Oak Road, Withington, Manchester.*

1899, Feb. 7. Chapman, D. L., M.A., Fellow of Jesus College, Oxford. *Jesus College, Oxford.*

1901, Nov. 26. Chevalier, Reginald C., M.A., Mathematical Master at the Manchester Grammar School. 17, *Central Road, West Didsbury, Manchester.*

1907, Nov. 26. Clayton, Robert Henry, B.Sc., Chemist. 1, *Parkfield Road, Didsbury, Manchester.*

1895, April 30. Collett, Edward Pyemont. 8, *St. John Street, Manchester.*

1911, May 9. Cook, Gilbert, M.Sc., A.M.Inst.C.E., Vulcan Research Fellow in Engineering in the University of Manchester. 8, *Clarendon Road, Garston, Liverpool.*

1903, Oct. 20. Core, William Hamilton, M.Sc. *Groombridge House, Withington, Manchester.*

1910, Oct. 18. Cotton, Robert, M.Sc., Demonstrator in Engineering in the University of Manchester. *Westholme, Devonshire Road, Pendleton, Manchester.*

1906, Oct. 30. Coward, H. F., D.Sc., Assistant Lecturer in Chemistry in the University of Manchester. *Municipal School of Technology, Sackville Street, Manchester.*

1906, Nov. 27. Coward, Thomas Alfred, F.Z.S. *Brentwood, Bowdon, Cheshire.*

1908, Nov. 3. Cramp, William, M.Sc.Tech., M.I.E.E., Consulting Engineer. 20, *Mount Street, Manchester.*

1910, Oct. 4. Crewe, F. H., Assistant Science Master in the Municipal Secondary School, Whitworth Street. *Glengarth, Woodford Road, Bramhall.*

Date of Election.

1911, April 4. Darwin, C. G., B.A., Reader in Mathematical Physics in the University of Manchester. *The University, Manchester.*

1895, April 9. Dawkins, W. Boyd, M.A., D.Sc., F.R.S., Honorary Professor of Geology in the Victoria University of Manchester. *Fallowfield House, Fallowfield, Manchester.*

1894, Mar. 6. Delépine, A. Sheridan, M.B., B.Sc., Professor of Pathology in the Victoria University of Manchester. *The University, Manchester.*

1887, Feb. 8. Dixon, Harold Baily, M.A., M.Sc., F.R.S., F.C.S., Professor of Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1906, Oct. 30. Edgar, E. C., D.Sc., Assistant Lecturer and Demonstrator in Chemistry in the University of Manchester. *The University, Manchester.*

1910, Oct. 18. Evans, Evan Jenkin, B.Sc., Assistant Lecturer and Demonstrator in Physics in the University of Manchester. *The University, Manchester.*

1907, Nov. 26. Fiatters, Abraham, F.R.M.S. *Syddal Cottage, Bramhall, Cheshire.*

1908, Jan. 28. Fox, Thomas William, M.Sc.Tech., Professor of Textiles in the School of Technology, Manchester University. 15, *Clarendon Crescent, Eccles.*

1909, Mar. 23. Gee, W. W. Haldane, B.Sc., M.Sc.Tech., A.M.I.E.E., Professor of Pure and Applied Physics in the School of Technology, Manchester University. *Oak Lea, Whalley Avenue, Sale.*

1910, Nov. 29. Geiger, Hans, Ph.D., Research Student in the University of Manchester. 62, *Nelson Street, Manchester.*

1896, Nov. 17. Gordon, Rev. Alexander, M.A. *Summerville, Victoria Park, Manchester.*

1907, Oct. 15. Gravely, F. H., M.Sc. *Natural History Dept., Indian Museum, Calcutta.*

1907, Oct. 29. Gwyther, Reginald Felix, M.A., Secretary to the Joint Matriculation Board. 21, *Booth Avenue, Withington, Manchester.*

1911, Oct. 3. Hassé, H. R., M.A., M.Sc., Lecturer in Mathematics in the University of Manchester. 100, *Ladybarn Lane, Fallowfield, Manchester.*

1902, April 29. Herbert, Arthur M., B.A. *Bankwyn, Hale, Cheshire.*

1902, Jan. 7. Hewitt, David B., M.D. *Grove Mount, Davenham, Cheshire.*

Date of Election.

1907, Oct. 15. Hickling, H. George A., D.Sc., Assistant Lecturer and Demonstrator in Geology in the University of Manchester. *Glenside, Marple Bridge, near Stockport*

1895, Mar. 5. Hickson, Sydney J., M.A., D.Sc., F.R.S., Professor of Zoology in the Victoria University of Manchester. *The University, Manchester.*

1884, Jan. 8. Hodgkinson, Alexander, M.B., B.Sc. 18, *St. John Street, Manchester.*

1909, Jan. 12. Hoffert, Hermann Henry, D.Sc. (Lond.), A.R.S.M., His Majesty's Inspector of Schools. *Lime Grove, Brooklands, Sale.*

1909, Nov. 2. Holland, Sir Thomas H., K.C.I.E., D.Sc., F.R.S., Professor of Geology and Mineralogy in the University of Manchester, late Director of the Geological Survey of India. *Westwood, Alderley Edge, Cheshire.*

1905, Nov. 14. Holt, Alfred, M.A., D.Sc., Research Fellow of the University of Manchester. *Crofton, Aigburth, Liverpool.*

1898, Nov. 29. Hopkinson, Sir Alfred, K.C., M.A., LL.D., Vice-Chancellor of the Victoria University of Manchester. *Fairfield. Victoria Park, Manchester.*

1896, Nov. 3. Hopkinson, Edward, M.A., D.Sc., M.Inst.C.E. *Ferns, Alderley Edge, Cheshire.*

1909, Feb. 9. Howles, Frederick, M.Sc., Analytical and Research Chemist. 20, *Moxley Road, Crumpsall, Manchester.*

1889, Oct. 15. Hoyle, William Evans, M.A., D.Sc., F.R.S.E., Director of the Welsh National Museum, Cardiff. *City Hall, Cardiff.*

1907, Oct. 15. Hübner, Julius, M.Sc.Tech., F.I.C., Lecturer in the Faculty of Technology in the University of Manchester. *Ash Villa, Cheadle Hulme, Cheshire.*

1899, Oct. 17. Ingleby, Joseph, M.I.Mech.E. *Summer Hill, Pendleton, Manchester.*

1901, Nov. 26. Jackson, Frederick. 14, *Cross Street, Manchester.*

1911, Oct. 17. Jackson, W. H. 77, *Clarendon Road, Manchester.*

1870, Nov. 1. Johnson, William H., B.Sc. *Woodleigh, Altrincham.*

1911, Oct. 3. Johnstone, Mary A., B.Sc.(Lond.), Headmistress of the Municipal Secondary School for Girls, Whitworth Street, Manchester. 11, *Birchvale Drive, Romiley.*

1878, Nov. 26. Jones, Francis, M.Sc., F.R.S.E., F.C.S. *The Grammar School, Manchester.*

Date of Election.

1886, Jan. 12. Kay, Thomas. *Moorfield, Stockport, Cheshire.*

1910, Oct. 18. Kershaw, Edith May, M.Sc., Demonstrator in Botany in the University of Manchester. *Ash Meade, Upper Mill, Yorks.*

1903, Feb. 3. Knecht, Edmund, Ph.D., Professor of Chemistry in the School of Technology, Manchester University. *Beech Mount, Marple, Cheshire.*

1893, Nov. 14. Lamb, Horace, M.A., LL.D., D.Sc., Sc.D., F.R.S., Professor of Mathematics in the Victoria University of Manchester. 6, *Wilbraham Road, Fallowfield, Manchester.*

1909, Nov. 2. Lang, William H., D.Sc., M.B., C.M., F.R.S., Barker Professor of Cryptogamic Botany in the University of Manchester. 2, *Heaton Road, Withington, Manchester.*

1902, Jan. 7. Lange, Ernest F., M.I.Mech.E., F.C.S. *Fairholm*, 3, *Willow Bank, Fallowfield, Manchester.*

1911, Jan. 10. Lankshear, Frederick Russell, B.A. of N.Z. University, Demonstrator in Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1910, Oct. 18. Lapworth, Arthur, D.Sc., Lecturer in Chemistry in the University of Manchester. 30, *Amherst Road, Withington, Manchester.*

1904, Mar. 15. Lea, Arnold W. W., M.D. 246, *Oxford Road, Manchester.*

1903, Nov. 17. Leigh, Charles W. E., Librarian of the University. *The University, Manchester.*

1907, Oct. 29. Leigh, Harold Shawcross. *Brentwood, Worsley.*

1908, Oct. 20. Liebert, Martin, Ph.D., Managing Director of Meister Lucius, and Brüning, Ltd., Manchester. *Swinton House, Wilmslow Road, Withington, Manchester.*

1902, Jan. 7. Longridge, Michael, M.A., M.Inst.C.E. *Linkvretten, Ashley Road, Bowdon, Cheshire.*

1857, Jan. 27. Longridge, Robert Bewick, M.I.Mech.E. *Yew Tree House, Tabley, Knutsford, Cheshire.*

1866, Nov. 13. McDougall, Arthur, B.Sc. *Lyndhurst, The Park, Buxton.*

1910, Oct. 18. McDougall, Robert, B.Sc. *City Corn Mills, Germain Street, Manchester.*

1905, Oct. 31. McNicol, Mary, M.Sc. 182, *Upper Chorlton Road, Manchester.*

1904, Nov. 1. Makower, Walter, B.A., D.Sc. (Lond.), Lecturer in Physics in the University of Manchester. 214, *Upper Brook Street, Manchester.*

Date of Election.

1902, Mar. 4. Mandleberg, Goodman Charles. *Reacyffe, Victoria Park, Manchester.*

1910, Oct. 18. Mangan, Joseph, M.A., Lecturer in Economic Zoology in the University of Manchester. *The University, Manchester.*

1875, Jan. 26. Mann, J. Dixon, M.D., F.R.C.P. (Lond.), Professor of Medical Jurisprudence in the Victoria University of Manchester. 16, *St. John Street, Manchester.*

1901, Dec. 10. Massey, Herbert. *Ivy Lea, Burnage, Didsbury, Manchester.*

1864, Nov. 1. Mather, Sir William, P.C., M.Inst.C.E., M.I.Mech.E. *Iron Works, Salford.*

1910, Nov. 1. Matheson, Atherton Greville Ewing, Mining Engineer. *Guildhall Chambers, 38/40, Lloyd Street, Manchester.*

1873, Mar. 18. Melvill, James Cosmo, M.A., D.Sc., F.L.S. *Meole-Brace Hall, Shrewsbury.*

1894, Feb. 6. Mond, Robert Ludwig, M.A., F.R.S.E., F.C.S. *Winnington Hall, Northwich, Cheshire.*

1911, May 9. Moseley, Henry Gwyn Jeffreys, B.A., Lecturer in Physics in the University of Manchester. *Dunwood House, Withington, Manchester.*

1911, Oct. 3. Mumford, A. A., M.B. *Wilmslow Road, Withington, Manchester.*

1908, Jan. 28. Myers, William, Lecturer in Textiles in the School of Technology, Manchester University. *Acresfield, Gatley, Cheshire.*

1873, Mar. 4. Nicholson, Francis, F.Z.S. *The Knoll, Windermere, Westmorland.*

1900, April 3. Nicolson, John T., D.Sc., Professor of Engineering in the School of Technology, Manchester University. *Nant-y-Glyn, Marple, Cheshire.*

1884, April 15. Okell, Samuel, F.R.A.S. *Overley, Langham Road, Bowdon, Cheshire.*

1907, Oct. 29. Osborn, Theodore George Bentley, B.Sc., Lecturer in Economic Botany in the University of Manchester. *Windlehurst, Anson Road, Victoria Park, Manchester.*

1892, Nov. 15. Perkin, W. H., Sc.D., Ph.D., M.Sc., F.R.S., Professor of Chemistry in the Victoria University of Manchester. *The University, Manchester.*

Date of Election.

1901, Oct. 29. Petavel, J. E., D.Sc., F.R.S., Professor of Engineering in the Victoria University of Manchester. *The University, Manchester.*

1885, Nov. 17. Phillips, Henry Harcourt, F.C.S. *Lynwood, Turton, nr. Bolton, Lancs.*

1903, Dec. 15. Prentice, Bertram, Ph.D., D.Sc., Principal of the Royal Technical Institute, Salford. *Isca Mount, Manchester Road, Swinton.*

1911, Oct. 17. Pring, J. N., D.Sc., Lecturer and Demonstrator in Electro-Chemistry in the University of Manchester. *The University, Manchester.*

1901, Dec. 10. Ramsden, Herbert, M.D. (Lond.), M.B., Ch.B. (Vict.). *Sunnyside, Dobcross, near Oldham, Lancs.*

1888, Feb. 21. Rée, Alfred, Ph.D., F.C.S. 15, *Mauldeth Road, Withington, Manchester.*

1908, Nov. 3. Reekie, J. A., Manager of the Hayfield Printworks. *Buckton Grange, Stalybridge.*

1869, Nov. 16. Reynolds, Osborne, M.A., LL.D., F.R.S., M.Inst.C.E. *St. Decuman's, Watchet, Somerset.*

1910, Oct. 4. Rhead, E. L., M.Sc.Tech., F.I.C., Lecturer on Metallurgy at the Municipal School of Technology, Manchester. *Stonycroft, Polygon Avenue, Levenshulme, Manchester.*

1880, Mar. 23. Roberts, D. Lloyd, M.D., F.R.S.E., F.R.C.P. (Lond.). *Ravenswood, Broughton Park, Manchester.*

1911, Jan. 10. Robinson, Robert, D.Sc. (Vict.), Teacher of Chemistry in the Victoria University of Manchester. *Field House, Chesterfield.*

1910, Oct. 18. Rossi, Roberto, M.Sc., Student. *Dalton Hall, Victoria Park, Manchester.*

1897, Oct. 19. Rothwell, William Thomas. *Heath Brewery, Newton Heath, near Manchester.*

1907, Oct. 15. Rutherford, Ernest, M.A., F.R.S., Langworthy Professor of Physics in the University of Manchester. 17, *Wilmslow Road, Withington, Manchester.*

1911, Oct. 17. Sandiford, Peter, M.Sc., Ph.D. Lecturer and Demonstrator in Education in the University of Manchester. *The University, Manchester.*

1909, Jan. 26. Schmitz, Hermann Emil, M.A., B.Sc., Physics Master at the Manchester Grammar School. 15, *Brighton Grove, Rusholme, Manchester.*

Date of Election.

1873, Nov. 18. Schuster, Arthur, Sc.D., Ph.D., F.R.S., F.R.A.S., Honorary Professor of Physics in the Victoria University of Manchester. *Kent House, Victoria Park, Manchester.*

1898, Jan. 25. Schwabe, Louis. *Hart Hill, Eccles Old Road, Pendleton, Manchester.*

1908, Nov. 17. Schwartz, Alfred, A.K.C., M.Sc.Tech., M.I.E.E., Assoc.M.Inst.C.E., Professor of Electrical Engineering in the School of Technology, Manchester University. *Mourne Lodge, Buxton.*

1890, Nov. 4. Sidebotham, Edward John, M.A., M.B., M.R.C.S. *Erlesdene, Bowdon, Cheshire.*

1903, April 28. Sidebottom, Henry. *Woodstock, Bramhall, Cheshire.*

1908, Nov. 3. Smith, Charles Frederick, M.Sc.Tech., M.I.E.E., Lecturer in Electrical Engineering in the School of Technology, Manchester University. 10, *Athol Road, Alexandra Park, Manchester.*

1910, Oct. 4. Smith, Grafton Elliot, M.A., M.D., F.R.S., Professor of Anatomy in the University of Manchester. *The University, Manchester.*

1906, Nov. 27. Smith, Norman, D.Sc., Assistant Lecturer in Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1895, Nov. 12. Southern, Frank, B.Sc. 38, *Store Street, Manchester.*

1896, Feb. 18. Spence, David. *Lowood, Hindhead, Haslemere, R.S.O., Surrey.*

1901, Dec. 10. Spence, Howard. *Audley, Broad Road, Sale, Cheshire.*

1904, Nov. 1. Stansfield, Herbert, D.Sc. (Lond.), A.I.E.E., Assistant Lecturer and Demonstrator in Physics in the University of Manchester. *The University, Manchester.*

1911, Oct. 17. Start, Laura, Lecturer in Art and Handicraft in the University of Manchester. *Moor View, Mayfield Road, Kersal.*

1897, Nov. 30. Stromeyer, C. E., M.Inst.C.E. *Steam Users' Association,* 9, *Mount Street, Albert Square, Manchester.*

1910, Oct. 18. Tattersall, Walter Medley, D.Sc., Keeper of the Manchester Museum. *The Museum, University, Manchester.*

1895, April 9. Tatton, Reginald A., M.Inst.C.E., Engineer to the Mersey and Irwell Joint Committee. *Manor House, Chelford, Cheshire.*

Date of Election.

1893, Nov. 14. Taylor, R. L., F.C.S., F.I.C. *Municipal Secondary School, Whitworth Street*, and 4, *St. Werburgh's Road, Chorlton-c.-Hardy, Manchester.*

1906, April 10. Thewlis, Councillor J. H. *Daisy Mount, Victoria Park, Manchester.*

1911, Oct. 17. Thoday, D., M.A., Lecturer in Plant Physiology in the University of Manchester. *The University, Manchester.*

1911, Jan. 10. Thomson, J. Stuart, Ph.D. (Bern), Senior Demonstrator in Zoology in the Victoria University of Manchester. *The University, Manchester.*

1873, April 15. Thomson, William, F.R.S.E., F.C.S., F.I.C. *Royal Institution, Manchester.*

1896, Jan. 21. Thorburn, William, M.D., B.Sc. 2, *St. Peter's Square, Manchester.*

1896, Jan. 21. Thorp, Thomas, F.R.A.S. *Moss Bank, Whitefield, near Manchester.*

1911, Oct. 3. Todd, T. Wingate, M.B., Ch.B. Demonstrator in Anatomy in the University of Manchester. *The University, Manchester.*

1899, Oct. 17. Todd, William Henry. *Rivington, Irlam Road, Flixton, near Manchester.*

1909, Jan. 26. Varley, George Percy, M.Sc. (Vic.), Assistant Master in the Municipal Secondary School, Manchester. 18, *Victoria Road, Whalley Range, Manchester.*

1873, Nov. 18. Waters, Arthur William, F.L.S., F.G.S. *Alderley, McKinley Road, Bournemouth.*

1906, Nov. 13. Watson, D. M. S., M.Sc. 60, *Lissenden Mansions, Highgate Road, London, N.W.*

1892, Nov. 15. Weiss, F. Ernest, D.Sc., F.L.S., Professor of Botany in the Victoria University of Manchester. 30, *Brunswick Road, Withington, Manchester.*

1909, Feb. 9. Weizmann, Charles, Ph.D., D.Sc., Senior Lecturer in Chemistry in the University of Manchester. *The University, Manchester.*

1908, May 12. Welldon, Rt. Rev. J. E. C., D.D., Dean of Manchester. *The Deanery, Manchester.*

Date of Election.

1911, Oct. 17. West, Tom, B.Sc. Chemist and Metallurgist. *101, Spring Bank Street, Stalybridge.*

1907, Oct. 29. Whitehead, Thomas, B.Sc., Chemist to the Manchester Steam Users' Association. 89, *Kenworthy Street, Stalybridge.*

1901, Oct. 1. Wild, Robert B., M.D., M.Sc., M.R.C.P., Professor of Materia Medica and Therapeutics in the Victoria University of Manchester. *Broome House, Fallowfield, Manchester.*

1859, Jan. 25. Wilde, Henry, D.Sc., D.C.L., F.R.S. *The Hurst, Alderley Edge, Cheshire.*

1907, Oct. 15. Winstanley, George H., F.G.S., M.I.M.E., Lecturer in Mining Engineering and Mine Surveying in the University of Manchester. *Wigshaw Grange, Culcheth, near Warrington.*

1909, Jan. 26. Wolfenden, John Henry, B.Sc. (Lond.), A.R.C.S. (Lond.), Assistant Master in the Municipal Secondary School, Manchester. 13, *Pole Lane, Failsworth.*

1905, Oct. 31. Woodall, Herbert J., A.R.C.S. 32, *Market Place, Stockport.*

1860, April 17. Woolley, George Stephen. *Victoria Bridge, Manchester.*

1863, Nov. 17. Worthington, Samuel Barton, M.Inst.C.E., M.I.Mech.E. *Mill Bank, Bowdon,* and 37, *Princess Street, Manchester.*

1895, Jan. 8. Worthington, Wm. Barton, B.Sc., M.Inst.C.E. *Kirkstyles, Duffield, near Derby.*

N.B.—Of the above list the following have compounded for their subscriptions, and are therefore life members:—

Bailey, Charles, M.Sc., F.L.S.

Bradley, Nathaniel, F.C.S.

Brogden, Henry, F.G.S.

Ingleby, Joseph, M.I.Mech.E.

Johnson, William H., B.Sc.

Worthington, Wm. Barton, B.Sc.

HONORARY MEMBERS.

Date of Election.

1892, April 26. Abney, Sir W. de W., K.C.B., D.Sc., F.R.S. *Rathmore Lodge, Bolton Gardens South, South Kensington, London, S.W.*

1892, April 26. Amagat, E. H., For. Mem. R.S., Memb. Inst. Fr. (Acad. Sci.), Examinateur à l'École Polytechnique. *Avenue d'Orléans*, 19, *Paris.*

1894, April 17. Appell, Paul, Membre de l'Institut, Professor of Theoretical Mechanics. *Faculté des Sciences, Paris.*

1892, April 26. Ascherson, Paul F. Aug., Professor of Botany in the University of Berlin. *Universität, Berlin.*

1889, April 30. Avebury, John Lubbock, Lord, D.C.L., LL.D., F.R.S. *High Elms, Down, Kent.*

1892, April 26. Baeyer, Adolf von, For. Mem. R.S., Professor of Chemistry in the University of Munich. 1, *Arcisstrasse, Munich.*

1886, Feb. 9. Baker, John Gilbert, F.R.S., F.L.S. 3, *Cumberland Road, Kew.*

1889, April 30. Carruthers, William, F.R.S., F.L.S. 14, *Vermont Road, Norwood, London, S.E.*

1903, April 28. Clarke, Frank Wigglesworth, D.Sc. *United States Geological Survey, Washington, D.C., U.S.A.*

1866, Oct. 30. Clifton, Robert Bellamy, M.A., F.R.S., F.R.A.S., Professor of Natural Philosophy in the University of Oxford. 3, *Bardwell Road, Banbury Road, Oxford.*

1892, April 26. Curtius, Theodor, Professor of Chemistry in the University of Kiel. *Universität, Kiel.*

1892, April 26. Darboux, Gaston, Membre de l'Institut, Secrétaire perpétuel de l'Académie des Sciences, Doyen honoraire de la Faculté des Sciences. 3, *Rue Mazarine, Paris.*

Date of Election.

1894, April 17. Debus, H., Ph.D., F.R.S. 4. *Schlangenweg, Cassel, Hessen, Germany.*

1900, April 24. Dewar, Sir James, M.A., LL.D., D.Sc., F.R.S., V.P.C.S., Fullerian Professor of Chemistry at the Royal Institution. *Royal Institution, Albemarle Street, London, W.*

1892, April 26. Edison, Thomas Alva. *Orange, N.J., U.S.A.*

1895, April 30. Elster, Julius, Ph.D. 6, *Lessingstrasse, Wolfenbüttel.*

1900, April 24. Ewing, James Alfred, C.B., M.A., LL.D., F.R.S., Director of Naval Education to the Admiralty. *Royal Naval College, Greenwich.*

1889, April 30. Farlow, W. G., Professor of Botany at Harvard College. *Harvard College, Cambridge, Mass., U.S.A.*

1900, April 24. Forsyth, Andrew Russell, M.A., Sc.D., F.R.S.. *Trinity College, Cambridge.*

1892, April 26. Fürbringer, Max, Professor of Anatomy in the University of Heidelberg. *Universität, Heidelberg.*

1900, April 24. Geikie, James, D.C.L., LL.D., F.R.S., Murchison Professor of Geology and Mineralogy in the University of Edinburgh. *Kilmorle, Colinton Road, Edinburgh.*

1895, April 30. Geitel, Hans. 6, *Lessingstrasse, Wolfenbüttel.*

1894, April 17. Glaisher, J. W. L., Sc.D., F.R.S., Lecturer in Mathematics. *Trinity College, Cambridge.*

1894, April 17. Gouy, A., Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Physics in the University of Lyons. *Faculté des Sciences, Lyons.*

1900, April 24. Haeckel, Ernst, Ph.D., Professor of Zoology in the University of Jena. *Zoologisches Institut, Jena.*

1894, April 17. Harcourt, A. G. Vernon, M.A., D.C.L., F.R.S., V.P.C.S. *St. Clare, Ryde, Isle of Wight.*

1894, April 17. Heaviside, Oliver, F.R.S. *Homefield, Lower Warberry, Torquay.*

1892, April 26. Hill, G. W. *West Nyack, N. Y., U.S.A.*

1888, April 17. Hittorf, Johann Wilhelm, Professor of Physics at Münster. *Polytechnicum, Münster.*

Date of Election.

1892, April 26. Hooker, Sir Joseph Dalton, G.C.S.I., C.B., O.M., D.C.L., F.R.S., Corr. Memb. Inst. Fr. (Acad. Sci.). *The Camp, Sunningdale, Berks.*

1892, April 26. Klein, Felix, Ph.D., For. Mem. R.S., Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Mathematics in the University of Göttingen. 3, *Wilhelm Weber Strasse, Göttingen.*

1894, April 17. Königsberger, Leo, Professor of Mathematics in the University of Heidelberg. *Universität, Heidelberg.*

1902, May 13. Larmor, Sir Joseph, M.A., D.Sc., LL.D., Sec. R.S., F.R.A.S. *St. John's College, Cambridge.*

1892, April 26. Liebermann, C., Professor of Chemistry in the University of Berlin. 29, *Matthäi-Kirch Strasse, Berlin.*

1887, April 19. Lockyer, Sir J. Norman, K.C.B., F.R.S,, Corr. Memb. Inst. Fr. (Acad. Sci.). *Science School, South Kensington, London, S. W.*

1902, May 13. Lodge, Sir Oliver Joseph, D.Sc., LL.D., F.R.S., Principal of the University of Birmingham. *The University, Birmingham.*

1900, April 24. Lorentz, Henrik Anton, Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Physics in the University of Leyden. *Hooigracht*, 48, *Leyden.*

1892, April 26. Marshall, Alfred, M.A., formerly Professor of Political Economy in the University of Cambridge. *Balliol Croft, Madingley Road, Cambridge.*

1901, Apri 23. Metschnikoff, Élie, D.Sc., For. Mem. R.S. *Institut Pasteur Paris.*

1895, April 30. Mittag-Leffler, Gösta, D.C.L. (Oxon.), For. Mem. R.S., Professor of Mathematics in the University of Stockholm. *Djursholm, Stockholm.*

1894, April 17. Murray, Sir John, K.C.B., LL.D., D.Sc., F.R.S. *Challenger Lodge, Wardie, Edinburgh.*

1910, April 5. Nernst, Geh. Prof. Dr. Walter, Director of the Physikal-Chemisches Institut in the University of Berlin. *Am Karlsbad 26a, Berlin W. 35.*

Date of Election.

1902, May 13. Osborn, Henry Fairfield, Professor of Vertebrate Palæontology at Columbia College. *Columbia College, New York, U.S.A.*

1894, April 17. Ostwald, W., Professor of Chemistry. *Groszbothen, Kgr. Sachsen.*

1899, April 25. Palgrave, Sir R. H. Inglis, F.R.S., F.S.S. *Henstead Hall, Wrentham, Suffolk.*

1894, April 17. Pfeffer, Wilhelm, For. Mem. R.S., Professor of Botany in the University of Leipsic. *Botanisches Institut, Leipsic.*

1892, April 26. Poincaré, H., For. Mem. R.S., Membre de l'Institut, Professor of Astronomy in the University of Paris. 63, *Rue Claude Bernard, Paris.*

1892, April 26. Quincke, G. H., For. Mem. R.S., Professor of Physics in the University of Heidelberg. *Universität, Heidelberg.*

1899, April 25. Ramsay, Sir William, K.C.B., Ph.D., F.R.S., Professor of Chemistry in University College, London. 12, *Arundel Gardens, Notting Hill, London, W.*

1886, Feb. 9. Rayleigh, John William Strutt, Lord, O.M., M.A., D.C.L. (Oxon.), LL.D. (Univ. McGill), F.R.S., F.R.A.S., Corr. Memb. Inst. Fr. (Acad. Sci.). *Terling Place, Witham, Essex.*

1900, April 24. Ridgway, Robert, Curator of the Department of Birds, U.S. National Museum. *Brookland, District of Columbia, U.S.A.*

1897, April 27. Roscoe, Sir Henry Enfield, P.C., B.A., LL.D., D.C.L., F.R.S., V.P.C.S., Corr. Memb. Inst. Fr. (Acad. Sci.). 10, *Bramham Gardens, Earl's Court, London, S.W.*

1902, May 13. Scott, Dukinfield Henry, M.A., Ph.D., F.R.S., F.L.S. *East Oakley House, Oakley, Hants.*

1892, April 26. Solms, H., Graf zu, Professor of Botany in the University of Strassburg. *Universität, Strassburg.*

1886, Feb. 9. Strasburger, Eduard, D.C.L., For. Mem. R.S., Professor of Botany in the University of Bonn. *Universität, Bonn.*

1895, April 30. Suess, Eduard, Ph.D., For. Mem. R.S., For. Assoc. Inst. Fr. (Acad. Sci.), Professor of Geology in the University of Vienna. 9, *Africanergasse, Vienna.*

Date of Election.

1892, April 26. Thiselton-Dyer, Sir W. T., K.C.M.G., C.I.E., M.A., Sc.D., Ph.D., LL.D., F.R.S. Lately Director Royal Botanic Gardens, Kew. *The Ferns, Witcombe, Gloucester.*

1895, April 30. Thomson, Sir Joseph John, M.A., Sc.D., F.R.S., Professor of Experimental Physics in the University of Cambridge. 6, *Scrope Terrace, Cambridge.*

1894, April 17. Thorpe, Sir T. E., C.B., Ph.D., D.Sc., LL.D., F.R.S., V.P.C.S. *Imperial College of Science and Technology, S. Kensington, London, S.W.*

1894, April 17. Turner, Sir William, K.C.B., M.B., D.C.L., F.R.S., F.R.S.E., Professor of Anatomy in the University of Edinburgh. 6, *Eton Terrace, Edinburgh.*

1886, Feb. 9. Tylor, Edward Burnett, D.C.L. (Oxon), LL.D. (St. And. and McGill Univs.), F.R.S., Professor of Anthropology in the University of Oxford. *Museum House, Oxford.*

1894, April 17. Vines, Sidney Howard, M.A., D.Sc., F.R.S., Sherardian Professor of Botany in the University of Oxford. *Headington Hill, Oxford.*

1894, April 17. Warburg, Emil, Professor of Physics at the Physical Institute, Berlin. *Physikalisches Institut, Neue Wilhelmstrasse, Berlin.*

1894, April 17. Weismann, August, Professor of Zoology in the University of Freiburg. *Universität, Freiburg i. Br.*

1888, April 17. Zirkel, Ferdinand, For.Mem.R.S., Professor of Mineralogy in the University of Bonn. *Königstrasse 2a, Bonn am Rhein.*

Awards of the Dalton Medal.

1898. Edward Schunck, Ph.D., F.R.S.

1900. Sir Henry E. Roscoe, F.R.S.

1903. Prof. Osborne Reynolds, LL.D., F.R.S.

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1787-1789.	James Massey.
1789-1804.	Thomas Percival, M.D., F.R.S.
1805-1806.	Rev. George Walker, F.R.S.
1807-1809.	Thomas Henry, F.R.S.
1809.	*John Hull, M.D., F.L.S.
1809-1816.	Thomas Henry, F.R.S.
1816-1844.	John Dalton, D.C.L., F.R.S.
1844-1847.	Edward Holme, M.D., F.L.S.
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1860-1861.	James Prescott Joule, D.C.L., F.R.S.
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1864-1865.	Robert Angus Smith, Ph.D., F.R.S.
1866-1867.	Edward Schunck, Ph.D., F.R.S.
1868-1869.	James Prescott Joule, D.C.L., F.R.S.
1870-1871.	Edward William Binney, F.R.S., F.G.S.
1872-1873.	James Prescott Joule, D.C.L., F.R.S.
1874-1875.	Edward Schunck, Ph.D., F.R.S.
1876-1877.	Edward William Binney, F.R.S., F.G.S.
1878-1879.	James Prescott Joule, D.C.L., F.R.S.
1880-1881.	Edward William Binney, F.R.S., F.G.S.
1882-1883.	Sir Henry Enfield Roscoe, D.C.L., F.R.S.
1884-1885.	William Crawford Williamson, LL.D., F.R.S.
1886.	Robert Dukinfield Darbishire, B.A., F.G.S.
1887.	Balfour Stewart, LL.D., F.R.S.

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1888-1889.	Osborne Reynolds, LL.D., F.R.S.
1890-1891.	Edward Schunck, Ph.D., F.R.S.
1892-1893.	Arthur Schuster, Ph.D., F.R.S.
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1905-1907.	Sir William H. Bailey, M.I.Mech.E.
1907-1909.	Harold Baily Dixon, M.A., F.R.S.
1909-1911.	Francis Jones, M.Sc., F.R.S.E.
1911-	F. E. Weiss, D.Sc., F.L.S.

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MEMOIRS AND PROCEEDINGS

OF

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II. Notes on Scattering during Radio-active Recoil. By Walter Makower, M.A., D.Sc., and Sidney Russ, D.Sc. *With 2 Text-figs.* - - - - - - - - - - - - pp. 1—4.

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III. The Development of the Atomic Theory: (2) The Various Accounts of the Origin of Dalton's Theory. By Andrew Norman Meldrum, D.Sc. - - - - - - - - - pp. 1—12.

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Proceedings - - - - - - - - - - - - - - - pp. i.—x.

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